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# CLASSICAL PHILOLOGY

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search in the Languages, Literatures,  
History, and Life of Classical Antiquity

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# CLASSICAL PHILOLOGY

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# CLASSICAL PHILOLOGY

Volume XXII

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Number 4

## ON THE EARLY HISTORY OF LEONTIUS' TRANSLATION OF HOMER

BY JAMES BRUCE ROSS

### I

The following facts related to the early history of Leontius' translation of Homer are now established. In the winter of 1358-59, Petrarch, at Padua, made the acquaintance of Leontius Pilatus.<sup>1</sup> On August 18, 1360, Petrarch wrote to Boccaccio the letter *Var.* 25, in which Petrarch indicates knowledge of the arrangement that Leontius is to translate Homer in Florence for Boccaccio and others, offers his aid in securing a text formerly on sale at Padua, and promises to send his own text if the Paduan one is not available.<sup>2</sup> On October 9, 1360, Petrarch wrote to Homer the letter *Fam.* xxiv. 12, in which he announces with joy that the translation has actually begun.<sup>3</sup> In November, 1362, Boccaccio left Florence for Naples. He then expected to stay there indefinitely; but he left in April, 1363.<sup>4</sup> He went from

<sup>1</sup> P. de Nolhac, *Pétrarque et l'Humanisme* (Paris, 1907), II, 131. J. E. Sandys, *A History of Classical Scholarship* (Cambridge, 1908), II, 8-9, wrongly assigns Leontius' coming to Italy to the year 1360.

<sup>2</sup> Nolhac, p. 133.

<sup>3</sup> *Ibid.*, p. 156.

<sup>4</sup> G. Traversari, "Per l'autenticità dell' epistola del Boccaccio a Francesco Nelli," in *Giornale storico della letteratura italiana*, XLVI (1905), 100 ff. Traversari's argument, while sufficiently strong to establish his conclusion, is imperfect in three minor respects. He fails to utilize the phrase *cinque mesi* in Boccaccio's letter to Barbato, though this phrase can be used to confirm Traversari's argument that Boccaccio stayed in Naples only five months. Traversari does not adequately refute Koerting's argument that the phrase *longiore circuitu* in Petrarch's letter *Sen.* iii. 1 supports the conclusion that Boccaccio spent the period from May, 1362, to the spring of 1363 in travelling from

Naples to Venice, where he spent three months with Petrarch in the summer of 1363; and then returned to Florence.<sup>1</sup> At a subsequent date not hitherto adequately established Petrarch wrote to Boccaccio the letter *Sen.* iii. 6, in which he asks Boccaccio to copy and send him as soon as possible a certain part of the translation of the *Odyssey*. On December 14, 1365, Petrarch wrote to Boccaccio a note, now incorporated in *Sen.* v. 1, in which he acknowledges receipt of the passage requested, says that he is astonished to hear that Boccaccio has already dispatched to him all of the *Iliad* and part of the *Odyssey*, and promises to have the MS copied promptly on his return to his home, where he expects to find it waiting for him, and to return the MS promptly to Boccaccio.<sup>2</sup> On January 25, 1366, Petrarch wrote to Boccaccio the letter *Sen.* vi. 1, in which he says that the translation has not yet arrived.<sup>3</sup> At a subsequent date not hitherto established Petrarch wrote to Boccaccio the letter *Sen.* vi. 2, in which he says that the translation has finally reached him. After receiving it Petrarch had a scribe make a copy which is preserved in the Bibliothèque Nationale, the *Iliad* under the number 7880 and the *Odyssey* under the number 7881. On a fly leaf of the *Iliad* is the following note in Petrarch's hand:

Domi scriptus, Pataui ceptus, Ticini perfectus, Mediolani illuminatus, et ligatus 1369.<sup>4</sup>

My purposes in the present study are to determine the dates of certain events in this history not hitherto adequately established, and to discuss briefly certain related matters.

Naples to Venice. The phrase is merely comparative: it means only that Boccaccio's route to Venice was longer than it would have been if he had gone via Florence. Traversari ignores the argument of L. Tanfani, Niccolò Acciaiuoli (Florence, 1863), p. 142, to the effect that Boccaccio was in Messina in May, 1362. Tanfani's calculations are invalidated by his disregard of the two months which Boccaccio spent in the "Sentina." Moreover, Boccaccio says that on taking leave of Nelli he journeyed *con lento passo infino ad Adversa*—which he could hardly have done from Messina. F. Torraca, *Per la biografia di Giovanni Boccaccio* (Milan, 1912), pp. 100 ff., ignores the work of Traversari, which, however, carries data which show Torraca's conclusions on this point to be invalid.

<sup>1</sup> Petrarch *Sen.* iii. 1, written September 7, 1363.

<sup>2</sup> A. Foresti, "Quando il Petrarca fece le grandi giunte al Bucolicum?" in *Rendiconti del R. Istituto Lombardo di Scienze e Lettere*, LVII (1924), 468-80. G. Finsler, *Homer in der Neuzeit von Dante bis Goethe* (Leipzig, 1912), pp. 16-17, wrongly assigns this letter to the year 1360.

<sup>3</sup> Foresti, *loc. cit.* Nohac, pp. 156-57, wrongly assigns this letter to the year 1367.

<sup>4</sup> Nohac, p. 166.

## II. LEONTIUS' ARRIVAL IN FLORENCE AND THE BEGINNING OF THE TRANSLATION

Petrarch's letter *Var.* 25, written on August 18, 1360, contains the following passage:

Quod petis extremum est videlicet Homeri librum qui venalis erat Patavii, si, ut reris, emerim, tibi accomodem, quando, ut ais, alter ab olim mihi est, quem Leo noster tibi atque aliis studiosis conterraneis nostris e graeco in latinum vertat. Illum ego librum vidi sed neglexi, quod meo impar visus esset. Haberi autem facile poterit, illo agente qui mihi Leonis ipsius amicitiam procuravit, cuius apud illum efficaces erunt litterae, et ego meas adiiciam. Si is forte, nos frustratur liber, quod non suspicor, tunc meus praesto erit.<sup>1</sup>

This letter has the tone of an answer written promptly in order to facilitate an enterprise considered praiseworthy in the highest degree and desirable from Petrarch's as well as from Boccaccio's point of view. It is then reasonable to suppose that Boccaccio's request was made in July or August, 1360.

Petrarch's letter to Homer, written on October 9, 1360, contains the following passage:

Vir, iste si vixerit, totum te nobis reddet: et iam coepit. . . . Quod ad me attinet, etsi tanto hospite non me digner, tamen te vel graecum, vel qua licuit, latinum domi habeo, brevi, ut spero, totum habiturus, si Thessalus tuus coeptum peregerit.<sup>2</sup>

It follows that Leontius reached Florence by October, 1360, and that Petrarch sent the Paduan MS to Boccaccio before October 9.<sup>3</sup> It is further probable that Leontius began the translation immediately on the receipt of the MS, which presumably reached Florence in September or October, 1360.

From the tone of *Var.* 25, written on August 18, it is probable that Leontius was already in Florence at that time. Moreover, it is inherently probable that Boccaccio would have waited until Leontius was actually in Florence before taking steps to procure a MS of Homer. This probability is strengthened by the fact that Boccaccio probably

<sup>1</sup> *Epistolae de rebus familiaribus et variis*, ed. by G. Fracassetti (Florence, 1859-63), III, 369.

<sup>2</sup> *Ibid.*, pp. 293, 304.

<sup>3</sup> That the MS used for the translation was the Paduan one and not Petrarch's text is proved by the quotation from the letter to Homer and also by Boccaccio's statement in the *Genealogia deorum*, XV, 7: "Nonne . . . ipse insuper fui, qui primus meis sumptibus omeri libros . . . in etruriam reuocaui . . . ?" I quote from O. Hecker, *Boccaccio-Funde* (Brunswick, 1902), pp. 277-78.

knew from Petrarch the uncertain and changeable character of Leontius' temperament. Since Boccaccio's request was perhaps made as early as July, 1360, it follows that Leontius may have arrived in Florence the same month. His arrival probably antedated Boccaccio's request for a text by only a few days, since there was no reason for delay and much reason for haste on Boccaccio's part. Leontius, then, probably reached Florence in June or July, 1360. It is inherently improbable that he should have come in the early months of the year, since the passes over the Apennines were little used in the winter months.<sup>1</sup>

The supposition that Leontius came to Florence in the spring of 1359 with Boccaccio on the latter's return from his visit to Petrarch in Milan is without adequate foundation. This supposition rests on Boccaccio's words:

Nonne ego fui, qui leontium pylatum auenetijs occiduam babilonem querentem alonga peregrinatione meis flexi consilijs, et in patria tenui . . . ?<sup>2</sup>

There is nothing in these words, however, to indicate that Boccaccio in person persuaded Leontius to come to Florence and took him with him on his return. The words *meis flexi consilijs* could easily refer to persuasion by letter.

We may then conclude that Leontius arrived in Florence in 1360, probably in June or July, certainly by October; and that the translation was begun at the end of August or later, probably in September or October.

### III. BOCCACCIO AND LEONTIUS

Boccaccio in the *Genealogia deorum* makes two statements of a general character as to the length of his relations with Leontius. The first is in XV, 6:

Nam eum legentem omerum et mecum singulari amicitia conversantem fere tribus annis audivi.<sup>3</sup>

It is probable, as we have seen, that Leontius reached Florence in June or July, 1360, and that the translation was begun in September or October. Boccaccio left Florence in November, 1362. In view of these circumstances it is probable that Boccaccio in using the words *fere tribus annis* is referring to the whole period of his relationship with

<sup>1</sup> Hecker, *op. cit.*, p. 272, n. 10.

<sup>2</sup> *Ibid.*, p. 277.

<sup>3</sup> *Ibid.*, p. 273.



Leontius and not merely to the period of the translation. The words refer to *mecum singulari amicitia conversantem* as well as to *eum legentem omerum*, and these two phrases are not necessarily synonymous.

Boccaccio's application of the words *fere tribus annis* to a period which began probably in June or July, 1360, and ended in October, 1362, is perfectly natural. He had every reason to exaggerate the length of time which Leontius spent with him, since he could thereby increase the measure of his martyrdom and his credit in the affair of the translation.<sup>1</sup>

In the *Genealogia*, XV, 7, in a passage quoted in part above, Boccaccio asks:

Nonne ego fui, qui leontium pylatum auenetijs occiduam babilonem querentem alonga peregrinatione meis flexi consilij, et in patria tenui, qui illum in propriam domum suscepi et diu hospitem habui, et maximo labore meo curavi, ut inter doctores florentini studij susciperetur, ei ex publico mercede apposita?

The *diu* of this passage evidently refers only to the period during which Leontius was Boccaccio's house-guest. The commonly repeated statement that Leontius was Boccaccio's house-guest for a period of three years results from an unjustifiable equation of the *diu* with the *fere tribus annis* of the other passage.

Boccaccio left Florence in November, 1362, without sending the translation to Petrarch. What became of it? Obviously, it was either taken to Naples with Boccaccio or left in Florence. If Boccaccio had taken the translation with him to Naples, he would have had it with him when he reached Venice. In that case he could have given all or part of it to Petrarch in Venice. Since there is no evidence that Petrarch received any part of the translation at this time, it is probable that Boccaccio left the translation in Florence and did not take it with him to Naples.

Why not? and why did he not send the translation to Petrarch before leaving Florence? There seem to be two possibilities: either

<sup>1</sup> Hecker, *op. cit.*, p. 272, n. 10, cites an example from the *De casibus virorum illustrium*, IX, 6, as a warning against the literal interpretation of expressions of time with *fere*. Boccaccio here writes "ex apice pontificatus, cum duobus annis et septem mensibus . . . (Johanna papa femina) sedisset," and shortly afterward, referring to the same event, "fere per triennium (papatum) possedit."

the translation was not completed when Boccaccio left, or, if completed, it was not in a form intelligible to Petrarch. There is no definite proof that Leontius had completed the translation by November, 1362; but since Petrarch later received a complete translation, Leontius must in any case have completed the translation before his departure from Italy in the autumn of 1363. If the translation was completed by November, 1362, it may well be that, the original MS of the translation being in a rough state, Boccaccio left it in Florence with the intention of having it copied by one of the friends who had been associated with him during the process of the translation. The theory that a copy of the translation was to be made for Petrarch receives confirmation from a passage in *Sen.* iii. 6, in which Petrarch, after asking Boccaccio to copy for him a passage of the *Odyssey*, adds:

In futurum autem si me amas, uide obsecro, an tuo studio, mea impensa fieri possit, ut Homerus integer bibliothecae huic, ubi pridem Graecus habitat, tandem Latinus accedat.<sup>1</sup>

The expression *tuo studio, mea impensa*, would seem to refer to a copy of the translation which Boccaccio was making or having made for Petrarch at the latter's expense.<sup>2</sup>

#### IV. THE DATE OF *SEN.* III. 6

This is the letter in which Petrarch asks Boccaccio to copy and send him as soon as possible a certain part of the translation of the *Odyssey*. It is dated by Petrarch March 1, without note of the year. The passage in question is as follows:

Postremo autem ne amici uolatilis, tam uerbosa mentio frustra sit, redit his in animum, te precari, ut Homericae partem illam *Odysseae*, qui Vlyxes it ad inferos, et locorum qui in uestibulo haerebi sunt, descriptionem ab Homero factam, ab hoc autem, de quo agimus, tuo hortatu in Latinum uersam, mihi quamprimum potes admodum egenti, utcumque tuis digitis exaratam, mittas, hoc in praesens. In futuro autem si me amas, uide obsecro, an tuo studio, mea impensa fieri possit, ut Homerus integer bibliothecae huic, ubi pridem Graecus habitat, tandem Latinus accedat.<sup>3</sup>

<sup>1</sup> Petrarch, *Opera* (Basle, 1554), p. 858.

<sup>2</sup> The question of the expenses involved in the translation has been discussed by a number of scholars. For a résumé of the arguments given, see O. Zenatti, *Dante e Firenze* (Florence, 1902), pp. 283 ff.

<sup>3</sup> Petrarch, *Opera*, p. 858.

The best discussion of the date of this letter is by Rossi,<sup>1</sup> who is inclined to accept 1364 as the correct date since Petrarch here refers to Boccaccio's departure after his visit in the summer of 1363 and to Leontius' departure in the autumn:

Hinc Leo, inquam, undecunque magna bellua, me nolente, frustraue diu ac multum dissuadente, surdior scopulis, ad quos ibat, tuum post abitum hinc abiit.<sup>2</sup>

This consideration seems to me to warrant a more positive conclusion than that reached by Rossi, and to prove beyond doubt that *Sen.* iii. 6 was written in 1364.

Rossi, however, though dating the letter as a whole 1364, advances the theory that the final part requesting the passage from the *Odyssey* should be dated later, in order to shorten the interval between the request for this passage and Petrarch's acknowledgment of its receipt in *Sen.* v. 1, written on December 14, 1365. He believes that the final part of *Sen.* iii. 6 is a fragment of another letter of much later date which was added to the letter of March 1, 1364, or an addition to that letter made

o quando la lettera, molto tempo dopo essere stata scritta, fu spedita o, smarritasi, rispedita, oppure quando entrò a far parte della collezione delle *Senili*.<sup>3</sup>

Rossi has no evidence to support such a theory. It is true that we have an example of a "contaminated" letter in *Sen.* v. 1; but the fact of that contamination cannot be used to prove that of another letter without specific proof in the latter case. The possibilities of delay in the transmission of *Sen.* iii. 6 and in the copying and transmission of the *Odyssey* passage and the possibility that Petrarch's acknowledgment is considerably later than his receipt of the passage are readily available in explanation of the long interval between *Sen.* iii. 6 and *Sen.* v. 1. It seems to me that, lacking evidence of any later date for the final part of *Sen.* iii. 6, we must accept March 1, 1364, as the date of the entire letter.

<sup>1</sup> V. Rossi, "Il Petrarca a Pavia," in *Bollettino della Società Pavese di Storia Patria* (Pavia, 1904), IV, 416.

<sup>2</sup> Petrarch, *Opera*, p. 857.

<sup>3</sup> Rossi, "Il Codice Lat. 8568 della Biblioteca Nazionale di Parigi e il testo delle 'Familiari' del Petrarca," in *Memorie della R. Accademia dei Lincei*, XVI (1920), 210, n. 1.

## V. SEN. VI. 2

On January 25, 1366, as has been said, Petrarch wrote to Boccaccio the letter *Sen. vi. 1*, in which he says that the translation has not yet arrived. At a subsequent date, Petrarch wrote to Boccaccio the letter *Sen. vi. 2*, in which he says that the translation has arrived.

In all previous discussions of the date of this letter it has been assumed that it was written shortly after *Sen. vi. 1*.<sup>1</sup> There is, however, no justification for such an assumption. The nearest semblance of evidence for it is Petrarch's mention in it of the death of Leontius, which he had also mentioned in *Sen. vi. 1*. This carries no real weight, however, for it is obvious that the receipt of the translation, however late it might be, would inevitably bring to Petrarch's mind the memory of Leontius and his fate.

Nor does the letter contain internal evidence, other than the reference to the translation, which can be used for a satisfactory determination of its date. It contains a defense of Petrarch's habit of spending much of his time at a despot's court. But his relation with Gian Galeazzo Visconti, which is probably the relation in question, lasted from before 1366 to 1368 or later.<sup>2</sup>

The letter then is not immediately datable, and does not in itself yield evidence as to the date of Petrarch's receipt of the translation.

## VI. PETRARCH'S KNOWLEDGE OF HOMER

It would seem that some light might be thrown upon the question of the date of Petrarch's receipt of the translation by a consideration of the dates of the letters and other writings of Petrarch in which he makes use of Homeric material.

In the first column of the following chart I have listed the passages which Nollac<sup>3</sup> has indicated as showing direct or indirect acquaintance with Homer. In the second column I have given the probable dates of these passages, so far as they are ascertainable, as indicated in the various works referred to.

<sup>1</sup> Fracassetti, in his translation of the *Lettere senili* (Florence, 1869-70), I, 327; Rossi, "Il Petrarca a Pavia," p. 415; Foresti, "Quando il Petrarca fece le grandi giunte al Bucolicum?" p. 473; Foresti, "Giovanni da Ravenna e il Petrarca," in *Commentari dell' Ateneo de Brescia*, 1923, p. 176, n. 2.

<sup>2</sup> Rossi, "Il Petrarca a Pavia," pp. 398-408.

<sup>3</sup> II, 167-69.



## PASSAGES IN PETRARCH INDICATING KNOWLEDGE OF HOMER

References to the <i>Iliad</i>	Probable Date
<i>De ignorantia</i> . . . . .	End of 1367 <sup>1</sup>
<i>De remediis</i> . . . . .	Early October, 1366 <sup>2</sup>
<i>Ep. Met.</i> II. 14. v. 34 ff. . . . .	1346-47 <sup>3</sup>
<i>Res memorandae</i> iv. 5. . . . .	1344-45 <sup>4</sup>
<i>Sen.</i> vi. 8. . . . .	
References to the <i>Odyssey</i>	
<i>Africa</i> iii. 375-76. . . . .	
<i>Sen.</i> i. 5. . . . .	1362 <sup>5</sup>
<i>Sen.</i> iv. 5. . . . .	
<i>Sen.</i> vi. 8. . . . .	
<i>Sen.</i> viii. 3. . . . .	November 9, 1367 <sup>6</sup>
<i>Sen.</i> ix. 1. . . . .	Late 1367 or early 1368 <sup>7</sup>
<i>Sen.</i> xii. 2. . . . .	1370 <sup>8</sup>
<i>Sen.</i> xv. 3. . . . .	1373 <sup>9</sup>
References to Homer	
<i>Ep. Met.</i> ii. 11. 234 ff. . . . .	Before January, 1349 <sup>10</sup>
<i>Res mem.</i> iii. 2 <sup>11</sup> . . . . .	
<i>Secretum</i> iii <sup>12</sup> . . . . .	
Sonnet <i>Se Virgilio ed. Homero</i> . . . . .	
<i>Trionfo della fama</i> , ii. 17 ff. . . . .	Early 1369 <sup>13</sup>
<i>Vita solitaria</i> ii. 7. 2 <sup>13</sup> . . . . .	

It is to be borne in mind that Petrarch had no direct knowledge of Homer until the winter of 1358-59, when Leontius translated for him the first part—about five books—of the *Iliad*; and that he could

<sup>1</sup> L. M. Capelli, *Pétrarque, le Traité De sui ipsius et multorum ignorantia* (Paris, 1906), pp. 4-6.

<sup>2</sup> Rossi, "Il Petrarca a Pavia," p. 419.

<sup>3</sup> D. Magrini, *Le epistole metriche di Francesco Petrarca* (Rocca S. Casciano, 1907), p. 114.

<sup>4</sup> G. Volpi, *Il Trecento* (Milano, s.d.), p. 52.

<sup>5</sup> *Lettere senili*, I, 32.

<sup>6</sup> Rossi, *op. cit.*, p. 420.

<sup>7</sup> H. Cochin, "La grande controverse de Rome et Avignon au XIV<sup>e</sup> siècle," in *Études Italiennes*, II (1921), 6.

<sup>8</sup> *Lettere senili*, II, 268.

<sup>9</sup> *Ibid.*, p. 407.

<sup>10</sup> Magrini, p. 114.

<sup>11</sup> According to Nohac, II, 167, n. 2, the Homeric material here is second-hand, probably derived from Cicero.

<sup>12</sup> Petrarch, *Trionfi* (ed. by Calcaterra) (Rome, 1923), p. xxiv, n. 1.

<sup>13</sup> According to Nohac, II, 168, n. 4, the Homeric material here is certainly derived from Cicero.

not have had direct knowledge of the rest of Homer prior to January, 1366—the date of *Sen.* vi. 1.

The acquaintance indicated by the third, fourth, fifth, and tenth of the passages dated here must then have been second hand, although Nollhac thought it first hand. It is quite possible that the acquaintance indicated in some of the other dated passages is also second hand. Still, the following chronological series of passages and dates is noteworthy:

<i>De remediis</i> .....	Early October, 1366
<i>Sen.</i> viii. 3 .....	November 9, 1367
<i>De ignorantia</i> .....	End of 1367
<i>Sen.</i> ix. 1 .....	Late 1367 or early 1368

The only conclusion that can safely be drawn from this evidence is that Petrarch probably had the MS of Homer during the last few months of 1367. Since the 1366 date is not reinforced by any other near date we are not justified in accepting it as indication that Petrarch had the MS at that time. The foregoing conclusion does not prove that Petrarch did not receive the MS before late 1367; it merely indicates that such may have been the case.

#### VII. THE TRANSCRIPTION OF THE TRANSLATION

Petrarch's main movements in the years 1366–68 were, according to Rossi,<sup>1</sup> as follows. In 1366 he arrived in Pavia in the first half of July and remained there until after December 10. In 1367 he left Venice, his established place of residence, earlier than usual, arriving in Pavia before the end of May. About the middle of November, he left Pavia, and after a short stay in Venice, probably in order to settle his affairs there, he established his residence in Padua. In 1368, toward the end of April, he journeyed to Udine to greet the Emperor, Charles IV, returning to Padua early in May. On May 25 he left for Pavia, where he arrived on May 30. Early in June he went to Milan to be present at the wedding of Violante, daughter of Gian Galeazzo. He stayed in Milan a month, returning to Pavia on July 3. About July 15 he left Pavia for his home in Padua.

During these same years occurred the "rebellion," first departure, return, and final departure of Petrarch's favorite scribe, Giovanni da Ravenna, who made for him his copy of the translation of Homer. The chronology of this interesting series of events, as established by

<sup>1</sup> "Il Petrarca a Pavia," pp. 398–408.

Foresti<sup>1</sup> is as follows. The first signs of rebellion appeared on April 21, 1367, Petrarch being then at Padua (presumably for his frequent Eastertide visit). A few days later Petrarch returned to Venice; and shortly after that return Giovanni departed to seek his fortune. On Petrarch's arrival in Pavia late in May, however, he found the boy waiting for him, travel-worn and repentant. Giovanni's second and final departure occurred about a year later. This fact is proved by a passage from *Sen.* xi. 9, a letter which all scholars agree was written on the occasion of the second departure:

Hunc ego impetum iam per anni spatium non sine ingenio ac labore continui . . . nec iam ulla vel vi vel arte compesci potest.<sup>2</sup>

The copy of the translation written by Giovanni is preserved, as has been stated, in the Bibliothèque Nationale, and bears the inscription:

Domi scriptus, Pataui ceptus, Ticini perfectus, Mediolani illuminatus, et ligatus 1369.

Nolhac's<sup>3</sup> examination of this copy shows that the transcription fell into three blocks. The first and largest block includes the volume of the *Iliad* and the first few pages of the *Odyssey*. The second, indicated by a change of pen and ink, continues until the beginning of the last canto of the *Odyssey*. The third and last block is marked by a complete change in the character of the writing—although the hand is evidently the same. The scribe hurries, multiplies the number of abbreviations, and writes *s* without raising his pen; he is evidently determined to finish as soon as possible.

Scholars have differed as to the relation between these blocks of transcription and the movements of Petrarch and Giovanni. I shall state and criticize the theories of Foresti<sup>4</sup> and Rossi,<sup>5</sup> and shall then propose a third theory.

Foresti's theory is that Giovanni began his task at Padua early in 1367 and continued until he rebelled in April, and that during this time he did the first block of the transcription; that after his reunion with Petrarch in Pavia at the end of May he resumed work, doing the second block of the transcription; and that in the following spring, not long before his final departure, he discarded his usual care and wrote the third block as fast as he could. Foresti thinks that the

<sup>1</sup> "Giovanni da Ravenna e il Petrarca."

<sup>2</sup> Foresti, *op. cit.*, p. 179.

<sup>3</sup> *Op. cit.*, I, 118.

<sup>4</sup> "Giovanni da Ravenna e il Petrarca."

<sup>5</sup> "Il Petrarca a Pavia," pp. 407-8.

phrase *domi scriptus* means simply that the copy was made *in casa* and not by a copyist outside Petrarch's house.

Foresti's argument is weak in several points. In the first place, the words *domi scriptus* imply that the transcription was done in Petrarch's permanent place of residence. According to Rossi, Petrarch's permanent home in 1367 was in Venice until the end of the year. Therefore Petrarch's presence in Padua in April, 1367, must have been only temporary.<sup>1</sup> It does not seem probable that Petrarch, on the occasion of a short visit to Padua, would have had his scribe begin the transcription of Homer. Therefore, since the copy was *domi scriptus*, *Pataui ceptus*, it was probably not begun in Padua early in 1367.

In the second place, it appears improbable that the first and largest block of the transcription should have been done in less than four months—from January (or later) to April, 1367—while the second block took nearly a year.

There is, furthermore, another piece of evidence concerning the work on which Giovanni was engaged in December, 1366, and probably during the first months of 1367, which impugns Foresti's theory.

Nolhae discovered, by a comparison of facsimiles, that the scribe of the Homeric poems was identical with the scribe of the non-autograph portion of the final MS of the *Canzoniere*, V.L. 3195.<sup>2</sup> From a consideration of one of the preliminary sheets of poems now assembled as V.L. 3196, we can determine approximately the date on which Giovanni quit work on the *Canzoniere*. The upper right-hand corner of f. 1r of this set of sheets bears this note in Petrarch's hand: "1366. Sabato ante lucem. decembris 5."<sup>3</sup>

Dr. Wilkins in a forthcoming study<sup>4</sup> distinguishes seven periods in Petrarch's entry of poems on the sheet in question. In the first period, Petrarch entered the poems which appear on f. 1r; in the second period, he entered the poem *Almo sol* on f. 1v; in the third period he entered the next few poems; etc.

It is evident that some time intervened between the first and second periods. While there is no great dissimilarity in script between the

<sup>1</sup> This conclusion is strengthened by Foresti's own interpretation of the phrase "usque ad meum reditum" in *Sen.* v. 5. If Petrarch refers in these words to his return to Venice, that city must have been his real home at the time.

<sup>2</sup> *Op. cit.*, I, 118.

<sup>3</sup> C. Appel, *Zur Entwicklung italienischer Dichtungen Petrarca's* (Halle, 1891), p. 22.

<sup>4</sup> "On the Dates of the Transcription of Petrarch's MS V.L. 3195."



first and second periods, there is a dissimilarity in ink. Furthermore, some time must have intervened between Petrarch's entry of *Almo sol* in the second period and the copying of that poem into V.L. 3195 by the scribe; for beside it in V.L. 3196 stands a notation in Petrarch's hand, but in an ink different from that of the poem, which was intended to guide the scribe in the location of the poem. Furthermore, after transcribing *Almo sol* in V.L. 3195 Giovanni transcribed also Nos. 305-18. It is then certain that the end of Giovanni's work on the *Canzoniere* is subsequent by a considerable amount of time, presumably two months or so, to December 5, 1366.

It is improbable that Petrarch would have set Giovanni to work transcribing Homer while he was still engaged on the *Canzoniere*. Therefore, since the earliest probable date on which Giovanni stopped work on the *Canzoniere* is considerably later than December 5, 1366, say February, 1367, it is improbable that he should have begun transcribing Homer until after that date. However, since it is improbable that in the interval between February and late April, 1367, Giovanni could have copied all of the *Iliad* and the first sheets of the *Odyssey*, and since it is perfectly possible that Giovanni's work on the *Canzoniere* may have continued more than two months after December 5, 1366, it appears highly improbable that Giovanni began the transcription of Homer before his first departure in late April, 1367. It seems reasonable to believe that at the time of the first departure Giovanni was engaged, not on the transcription of Homer, but still on the *Canzoniere*.

Rossi's theory is that the first and second blocks of transcription were both done in Padua during the first five months of 1368; and that the third block was done during Petrarch's short visit in Lombardy in June and July of that year.

This interpretation also has its weak points. If the bulk of the transcription was done in one place, Padua, it is difficult to explain the change of pen and ink in the first part of the *Odyssey*. Rossi does not consider this change of much importance, and certainly its significance should not be overemphasized; but it cannot be ignored. The most plausible explanation would be a change of residence; but it could be explained by a lapse of time or some unknown interruption. Another difficulty is the fact that Petrarch was in or near Lombardy from May 30 to July 15, a period too long to explain the apparent haste with which the last canto was transcribed.

My own theory is the following. Giovanni began the transcription late in November or in December, 1367, soon after Petrarch's establishment of his permanent residence in Padua. The progress of the transcription was interrupted in late May, 1368, when Giovanni had already completed the *Iliad* and the first pages of the *Odyssey*, by Petrarch's moving to Pavia. There the transcription was continued and finally completed. The last canto was probably transcribed just before Petrarch's departure for Padua, which was very possibly the occasion of Giovanni's final departure.

This theory accounts satisfactorily for the change of pen and ink in the early part of the *Odyssey*, but it also contains certain difficulties. In the first place, it allows a very short period of time for the transcription of the bulk of the *Odyssey*. In the second place, it demands a looser interpretation of the words *anni spatium* than that given by Foresti, who believes that the second flight occurred almost exactly a year after April, 1367, that is, in April, 1368. However, such an exact translation of a general expression of time like *anni spatium* is hardly justifiable. It is possible to regard *anni spatium* as meaning a period several months under or over a year; and it is not unreasonable, therefore, to suppose that Giovanni's second departure may have occurred as late as July, 1368.

All in all, we may certainly conclude that it is probable that the transcription was not begun until after the date of Petrarch's arrival in Padua in November or December, 1367.

#### VIII. THE DATES OF PETRARCH'S RECEIPT OF THE TRANSLATION AND OF SEN. VI. 2

We have still to consider the relation of the date of the beginning of the transcription to the date of the receipt of the MS from Boccaccio, and to the date of *Sen. vi. 2*, which was probably written shortly after the MS arrived. Foresti<sup>1</sup> believes that Petrarch, in spite of his promise in *Sen. v. 1* to transcribe and return to Boccaccio the MS when he should have received it, actually received a copy of Boccaccio's own MS of translation and not that MS itself. He thinks that Boccaccio did what he was requested to do in *Sen. iii. 6*, that is, sent Petrarch a copy of the translation which Petrarch might keep. Since Petrarch received a copy of the translation and not the original, he

<sup>1</sup> "Giovanni da Ravenna e il Petrarca," 172-73.

was under no pressure to have another copy made. His reason for having Giovanni make another copy was probably the desire to have a more luxurious Homer and one with wide margins for annotations. Since Petrarch was under no pressure to have a copy made, there can be little relation between the date of the receipt of the translation and the date of the beginning of the transcription.

As a matter of certainty we can only say that the MS was received after January 25, 1366, the date of *Sen.* vi. 1, and before the latest date possible as the date of the beginning of the transcription, namely the early part of 1368.

It is certainly noteworthy, however, that the evidence presented in Part VI of this paper suggests that Petrarch's first use of the MS was probably made late in 1367; and that independent consideration of the date of the beginning of the transcription indicates that the transcription probably began late in 1367.

This coincidence seems to me to warrant the conclusion that Petrarch probably received the MS late in 1367. It is obviously probable that his letter of acknowledgment, *Sen.* vi. 2, was written soon after the receipt of the MS—probably before the end of the year.

#### IX. SUMMARY

To the facts reported in Part I of this study the following conclusions may then be added. Leontius arrived in Florence in 1360, probably in June or July, certainly by October. The translation was begun at the end of August or later, probably in September or October. Petrarch's letter *Sen.* iii. 6, in which he requests a certain part of the translation of the *Odyssey*, was written on March 1, 1364. Petrarch received a copy of the complete translation at some time subsequent to January 25, 1366, and before the end of 1367, probably late in 1367. His letter of acknowledgment, *Sen.* vi. 2, was probably written late in 1367. The transcription by Giovanni was probably begun late in 1367 or early in 1368, more probably in 1367 than in 1368. The first block of the transcription was probably done in Padua in the early months of 1368, and the second and third blocks in Lombardy in late May, June, and early July.<sup>1</sup>

<sup>1</sup> I am indebted to President E. H. Wilkins of Oberlin College for the suggestion of this study, and for help in preparing it for publication.

## THE OVIDIAN AUTHORSHIP OF THE LYGDAMUS ELEGIES

BY ROBERT S. RADFORD

### I. INTRODUCTION

I have attempted in a recent volume of the *Transactions of the American Philological Association*<sup>1</sup> to make a study of the language of the six Lygdamus elegies and also to give a visual exhibit of two of these elegies (v and iii) in relation to Ovid. The present article seeks to examine a few of the many remarkable idioms which occur in the poems and also to present a complete visual exhibit of the first elegy.

Our problem may be stated as follows. The third book of Tibullus<sup>2</sup> contains a distich (v. 17-18), which describes the year of the author's birth as 43 B.C.:

natalem primo nostrum videre parentes,  
cum cecidit fato consul uterque pari.

From the time of Scaliger (1577) to that of Voss (1810), a period of more than two centuries, all classical scholars without exception regarded the birth line (vs. 18) as an interpolation introduced from the passage in the *Tristia* (iv. 10. 6), in which the poet Ovid gives his own birth-date. By means of this hypothesis they were able to retain Tibullan authorship of the third book. In the early nineteenth century (1810) Voss first advanced the view that "Lygdamus" was an entirely different poet from Tibullus, and a weak and contemptible imitator of Ovid. On the other hand, Gruppe and Kleemann maintained that Lygdamus was none other than Ovid himself, as shown both by the biographical details and the evidences of language. Unfortunately, however, Gruppe's *Römische Elegie* was published at a time (1838) when Lachmann and the followers of F. A. Wolf were engaged in tearing to shreds both the works of Homer and the *corpus* of Ovid, and when therefore all constructive work was at a discount. Kleemann's

<sup>1</sup> LVII (1926), 149-80.

<sup>2</sup> I use the term "third book" very loosely for the Lygdamus elegies. In reality all the poems which follow the second book of Tibullus belong to the third book and proceed from the same author.

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meritorious dissertation<sup>1</sup> is singularly incomplete and attracted little attention from the scholars of the period, who were still under the influence of Lachmann. Although Richter<sup>2</sup> urged that the whole subject should be taken up afresh, no one came forward to do so, and therefore since 1876 not a single voice has been raised in destructive Germany in defense of the once famous Messalla Collection. In France, however, two eminent scholars, Philippe-Aimé de Golbéry (1826) and Frédéric Plessis (1883, 1884, 1909), protested earnestly against the obloquy unjustly heaped upon the elegies and successfully vindicated their literary merits.<sup>3</sup> Furthermore the influence of Lachmann and his school has greatly diminished in recent years even in Germany, and for the first time since the appearance of Wolf's *Prolegomena* (1795) conditions are genuinely favorable for the study of the Ovidian problems which the restless nineteenth century so signally failed to solve.

I have spoken of the existence of "Ovidian problems." The question may be asked whether it is necessary to attempt their solution. I do not hesitate to reply in the affirmative, since the difficulties in question exist both without and within the Ovidian *corpus*. Lachmann, Haupt, Birt, and Skutsch were not wholly reckless and wanton destroyers. They believed in a "static Ovid"—an Ovid who had no development in his art. They saw that the *corpus* contains many works which are perfect in form and technique, but that there are also some poems within it which show many imperfections and irregularities. They therefore unhesitatingly rejected the *Halieutica*, the *Medicamen*, the *Consolatio*, and a large part of the *Heroides*, on the ground that they were too faulty to be the work of Ovid, the perfect artist. I may add that after the clear proof which I have given<sup>4</sup> of the grave defects which exist in the *Amores*, it will be the manifest duty of the followers of Lachmann to repudiate about a fourth of the *Amores* also. Let us also do justice to Lachmann's attempted solution of the Ovidian problem. It is no doubt now somewhat in disfavor, neverthe-

<sup>1</sup> *De libri tertii carminibus quae Tibulli nom. circumferuntur*, Strassburg, 1876.

<sup>2</sup> *Bursian's Jahresbericht*, X (1877), 284.

<sup>3</sup> The Latin style of the elegies, however—like that even of the mature Ovid, which I formerly praised much too highly (*Trans. Amer. Phil. Assoc.*, LI [1920], 165)—is somewhat careless and distinctly inferior to the pure Latinity of Tibullus.

<sup>4</sup> "Juvenile Works," *TAPA*, LI, 169.

less it is a genuine, though violent, attempt to remove the existing difficulties, and if it be rejected, the problem still remains. In opposition to Lachmann's view, I have brought forward elsewhere the conception of an Ovid who develops gradually in his art and reaches perfection only in middle life.<sup>1</sup> His dactylic technique, appearing as it does in a spondaic language, can only be the result of a long apprenticeship. In this way I am able not only to retain the defective poems which exist within the *corpus*, but also to make provisions for the many "furtive" and pseudonymous works—such as the Lygdamus elegies, the *Maecenas*, and the *Priapea*—which undoubtedly exist outside. Ovidian scholars cannot evade the issue indefinitely. In the end they must choose one or the other of the two conceptions and one or the other of the two solutions.

## II. CITATION OF EXCLUSIVELY OVIDIAN PHRASES

In quoting the following verses we mark by bold-faced type and by an asterisk (\*) those phrases which occur only in Ovid and in Lygdamus among all the poets of the Golden Age (Lucretius to Ovid). The dagger (†) similarly denotes an expression which is found in one other poet of the Golden Age only.<sup>2</sup> It is not meant, however, that all of the starred phrases occur in a single verse of Ovid, and we give express citations only for those particular phrases which we single out for discussion.<sup>3</sup> We may begin with Lygd. i. 12 f. and ii. 27:

\*indicet ut \*nomen \*littera \*facta meum,  
atque inter \*geminas pingantur cornua \*frontes

and

sed \*tristem \*mortis demonstret littera \*causam.

\**Littera* \**facta* in i. 12 is found in Ovid only (four times), but the chief feature of both verses is the peculiar use of *littera*. *Littera*, in its proper meaning, "(single) letter of the alphabet," occurs in Propertius, Lucretius, and the *Priapea* each once, and seven times in Ovid.<sup>4</sup>

<sup>1</sup> *Ibid.*, p. 146; see also now "Ovid's Carmina Furtiva," *Philological Quarterly*, Vol. VII (1928).

<sup>2</sup> In statistics the *Aetna* is often included in the Verg. App. (V.A.). The Tibullan Appendix is indicated by T.A., and includes also [Tib.] ii. 2, 3, and 5; see *AJP*, XLIV (1923), 3. Ovid's works contain nearly as many verses as all the other poets of the Golden Age combined.

<sup>3</sup> Many citations may be found in my article in *TAPA*, LVII, 163.

<sup>4</sup> *Nuz* 82; *Trist.* i. 11. 1; ii. 344; *Pont.* iv. 2. 24; *Ib.* 4; *Fast.* v. 482, 563.

Ovid, however, by far the most ingenious of Latin poets, uses *littera* fifty-four times for *litterae* (plur.), which the meter does not admit—and in the meanings “(several) letters,” “epistle,” “literary work,” “inscription.” This plural meaning is found besides only in *Priap.* xvi. 5 (“inscription”), *Cul.* 412, and here. In the *Culex* and also here *littera* has the meaning of “funereal inscription,” “epitaph”; cf. *Met.* xi. 706, *si non urna, tamen iunget nos littera*.<sup>1</sup> Ovid has *littera* besides in the general meaning of “inscription” five times: *A.A.* ii. 500; *Her.* xxi. 150, 238; xvii. 88; v. 26–30.

We may add i. 3 f. and i. 22:

et vaga nunc certa discurrunt undique \*pompa  
\*perque \*vias \*urbis munera perque domos

and

atque \*haec submisso \*dicite \*verba sono.

\*Pompa \*per \*vias occurs only in Ovid (*Am.* iii. 13. 12), just as \*pompa \*deum occurs only in *Lydia* 44 and *Am.* iii. 2. 61. The word *pompa* also is quite rare except in Ovid (24 *Ov.*, 1 *Lygd.*, 1 *V.A.*, 6 *al.*, 0 *Cat.*, *Hor.*, *Tib.*). Also the threefold phrase \*haec \*dicere \*verba (where *verba* is due to the meter) occurs only in *Met.* xiv. 717, *Lygd.* i. 22, and [*Tib.*] iv. 5. 18; cf. *verba* (nom. and acc. plur.): 317 *Ov.*, 11 *T.A.*, 2 *V.A.*, 65 *al.*+45 *Prop.*

We consider next iv. 84 and vi. 49 f.:

nec tantum \*crimen pectore \*inesse \*tuo

and

nulla \*fides \*inerit: \*periuria \*ridet \*amantum  
\*Iuppiter.

\*Crimen \*inesse \*tuo occurs twice in Ovid (*Her.* xvii. 218 and *Pont.* iii. 3. 70), and \*crimen \*inesse alone occurs once (*Pont.* iii. 1. 20); \*inesse \*fides occurs in *Her.* xvii. 130, and \*nullum \*inesse in *Pont.* iii. 3. 70. The verb is quite rare except in Ovid and Lucretius: 42 *Ov.*, 2 *Lygd.*, 1 *Aetn.*, 6 *al.*+13 *Lucr.*

We may cite also vi. 43 f.:

vos ego nunc moneo: †felix, †quicumque \*dolore  
\*alterius \*disces \*posse \*carere tuo

Ovid has \*dolore \*alterius once (*A.A.* i. 750) and \*disco ab \*alterius querelis once (*ibid.* iii. 455); he has \*disco \*posse \*carere once (*Her.*

<sup>1</sup> The poets usually express this meaning by *carmen*, *notae*, *versus*, or *titulus*; Ovid and his “imitators” alone uses *littera*.

xvii. 97) and *\*disco \*posse* twice (*ibid.* 90; *A.A.* iii. 296). As regards the twofold phrase *†posse †carere*, he has this seven times and Tibullus has it once.<sup>1</sup> We note also that *carere* is here the reading of the Ambrosian Codex, the great manuscript of Tibullus: "Happy are you who from another's pain shall learn successfully how to avoid your own." Recent editors, however, have all been disposed to introduce from inferior manuscripts the unusual syntax *cavere tuo*, "to guard against your own." But the usage of Ovid supports the Ambrosian Codex: For though he employs the infinitive *docere*, for example, only four times and has *cavere* only once, he makes a special feature of employing *carere* in verse-closes. Thus he uses it twenty-one times, while all the other poets together have *carere* only four times.<sup>2</sup> Ovid does not have *carere tuo*, but he has *carere mea* (*meum*, *mei*) four times.

Consider also iii. 17 and 19:

quidve in Erythraeo *\*legitur quae \*litore \*concha,*  
*\*et \*quae \*praeterea populus miratur? in illis. . . .*

*Praeterea* occurs in Vergil twenty-seven times, in Lucretius ninety-four times, but neither poet uses *\*et \*quae \*praeterea*. The latter is an individual idiom which Ovid uses five times,<sup>3</sup> in spite of his well-known inclination toward the dactyl in the first foot.

We pass next to iv. 95 and 96:

*\*haec \*deus in melius crudelia \*somnia vertat*  
*et \*iubeat \*tepidos irrita \*ferre \*Notos,*

compared with *Am.* ii. 8. 19 f.:

tu, *\*dea*, tu *\*iubeas* animi periuria puri  
 Carpathium *\*tepidos* per mare *\*ferre \*Notos.*

The phrase *ferre iubet* (*iube*) is common enough, but the subjunctive *\*iubeat* (*\*iubeas*) *\*ferre* occurs only in these two passages. The most astounding feature of the verses, however, is the locution *\*tepidi \*Noti*, which is found six times in Ovid<sup>4</sup> and nowhere else. The origin of the phrase has never been fully explained, but it is wholly individual and peculiar to Ovid. For *Notus* is designated by Vergil as

<sup>1</sup> Tib. i. 2. 64; *Am.* i. 7. 24 (*potui*); *Her.* i. 18; xvi. 286; xvii. 97; *Rem.* 540; *Pont.* i. 2. 66; iii. 7. 29.

<sup>2</sup> Verg. *Aen.* ii. 44; Hor. *Epod.* xvi. 16; Tib. i. 2. 64; Lucr. v. 1425.

<sup>3</sup> *Am.* iii. 7. 12, etc.

<sup>4</sup> *Ibid.* i. 4. 12; i. 7. 56; ii. 8. 20; *Her.* xi. 76; *Ib.* 34; *Pont.* iv. 10. 43. We may quote the first in full: "nec Euris | da mea, nec *\*tepidis verba \*ferenda \*Notis.*"

*sinister, violentus*, and *praeceps*, by Horace as *rapidus, invidus*, and *udus*, and by Propertius as *dubius, hibernus*, and *iratus*; Ovid is the only poet who has ever thought of applying to it the epithet *tepīdus*.<sup>1</sup>

Let us consider also vi. 21 and 24:

convenit iratus \*nimium \*nimiumque severos . . . .

\*Cadmeae \*matris \*praeda \*cruenta docet.

The phrase \*nimium \*nimiumque occurs also in *Her.* i. 41 (omitted by Kleemann). It is therefore wrongly designated by Draeger<sup>2</sup> as a ἀπαξ εἰρημένον in Latin; these are, however, the only two occurrences of the expression. In respect to verse 24, Ovid not only has \*praeda \*cruenta (*Fast.* v. 178), but he alludes again to the story of Pentheus and Agave with the similar phrase \*praeda \*matris *Thebanae* (*Fast.* iii. 721); cf. also *Met.* ix. 304, \*matres \*Cadmeides (more dactylic form), and *Cul.* 111, *Cadmeis Agave*.

We cite also vi. 61, 63, and 64:

(non) \*sollicitus \*repetam tota \*suspiria nocte . . . .

iam dudum Syrio madefactus \*tempora \*nardo

debueram \*sertis \*implicuisse \*comas.

The pluperfect \*debueram, with present or perfect infinitive, is itself a most striking Ovidianism, and is, like *littera*, chiefly a metrical device to increase the number of dactyls. It is not found elsewhere in the classical poets, but Ovid uses it twenty-four times.<sup>3</sup> A more normal use is the perfect *debuit* (*Prop.* iii. 5. 10), or the imperfect *debebat* (*Lucr.* i. 959). There is a single other case of the form *debueram* (*Verg. Aen.* x. 853), but it means "I had owed." Cf. also *Fast.* v. 220: \*sertaque caelestes \*implicitura \*comas. (We have, without *serta*, *Verg. Aen.* ii. 552, *implicuitque comam laeva*, "grasped with the hand.")

The Lygdamus poems, it is well known, are heavily spondaic (58.7 per cent), and quite unlike the *Priapea*, *Consolatio*, and *Maecenas*, they show few signs of the dactylic virtuosity attained in the years of our poet's full maturity. Yet not only must many of the elegant closes

<sup>1</sup> Kleemann, p. 58, is justly astounded: "Apud Vergilium et Horatium—haud raro apud Ovidium ipsum—Notus est gelidus et imbrifer ventus." *Tepīdus* is a favorite with Ovid: 44 *Ov.*, 1 *T.A.*, 1 *V.A.*, 12 *al.* + 8 *Verg.*

<sup>2</sup> *Ovid als Sprachbildner* (Aurich, 1886), p. 12.

<sup>3</sup> *Am.* iii. 12. 43; *Her.* iii. 40; vi. 4; xii. 4; xiii. 5; xx. 170; *Rem.* 112, 574; *ad Liv.* 292, 342; *Met.* vi. 700; ix. 591; ix. 602; ix. 729; xii. 445; *Trist.* iii. 4. 14 (*var. lect.*); iii. 13. 4; v. 3. 16; v. 12. 49 (*var. lect.*); *Ib.* 19; *Fast.* i. 354; vi. 591; *Trist.* iv. 4. 21 (*debuerim*); *Pont.* ii. 6. 12. Burman omits these examples.

both of hexameter and pentameter be repeated later by the mature poet, but it is also fully evident that the youthful Ovid must have already had in his possession—in a rudimentary and undeveloped way—some at least of the raw materials from which the stately structure of his dactylic verse was later to assume definite form.<sup>1</sup> I wish therefore to discuss next two striking verse-closes and cite for this purpose iv. 14 and iv. 91, 93:

et *\*frustra immeritum \*pertimuisse \*velit*

and

*\*barbara nec Scythiae \*tellus horrendave Syrtis, . . .  
et longe ante alias \*mitissima \*mater.*

It is well known that the elegiac poets like to place a quinquesyllabic perfect infinitive (here *pertimuisse*) in the pentameter close. This is especially characteristic of Ovid, who offers more than two hundred and forty such verses.<sup>2</sup> Though not usually expressly stated, it is also true that each poet has his own preferred and favorite verbs from which he derives the needed quinquesyllabic forms. Thus we find *Trist.* v. 9. 6:

*pars etiam credi \*pertimuisse \*velit.*

Not only have we the same identical verse-close, but among all the poets of the Golden Age *\*pertimui* is found only in Ovid (seventeen times), the *Ciris* (v. 82) and here (elsewhere only Plaut., Afran., Varr., and prose). But this is not all; for Lygdamus has also in this verse *frustra . . . pertimuisse*, "to fear without cause," and this locution is found among our poets only in Ovid; cf. *Am.* iii. 3. 23, *aut sine re nomen deus est \*frustraue \*timetur* (see also the *Thesaurus*, s.v. "frustra," p. 1434, l. 4).

With Lygd. iv. 93 above compare *Met.* vi. 118:

*et te flava comas frugum \*mitissima \*mater.*

Superlatives in *-issimus* in the fifth foot of the hexameter are very frequent in Vergil, but greatly restricted in the older elegiac poets (Eschenburg, p. 16). Ovid, however, gave to this usage an extension and a freedom which even Vergil had not developed. Thus *mitissimus* does not occur at all in the classical poets except in Ovid (fourteen

<sup>1</sup> What we may call "the mechanics" of Ovid's mature art have been most carefully studied by Zingerle, Eschenburg, and Ganszenmüller.

<sup>2</sup> Eschenburg, *Wie hat Ovid einzelne Wortklassen im Verse verwandt*, p. 29.



times) and in the present passage, while the example *Met.* vi. 118 is overlooked by Kleemann, and is not even in Burman's Index. Superlatives in *-issimus* belong to the Zingerle-Eschenburg tests; under these tests also fall *Lygd.* iv. 1, *di meliora ferant* (Kleemann, p. 59; Eschenburg, p. 3); *Lygd.* vi. 33, *ei mihi* (Kleemann, p. 59; Eschenburg, p. 2); *Lygd.* vi. 62, *i*, used for the more usual *i nunc* (Eschenburg, p. 3; Shuckburg on *Her.* iii. 26; and L. Müller on *Hor. Sat.* i. 10. 92).

We may consider next the remarkable use of *\*illud \*opus*, which cannot even be understood without reference to Ovid. In iv. 25 f. the apparition of Apollo is described. No age of olden time had ever seen a vision more lovely, nor any semblance of human form so fair:

*\*non \*illo \*quicquam\*formosius ulla priorum  
aetas, humanum nec videt \*illud \*opus.<sup>1</sup>*

This phrase seems so peculiar to nearly all recent editors (Hiller, Némethy, Postgate) that they adopt instead Lachmann's wholly needless emendation, *heroum nec tulit ulla domus*. But expressions of a similar peculiar kind—which no reader can easily forget—are applied by Ovid to Drusus in *ad Liv.* 39 and *Maec.* 150, and to Augustus in *Met.* xv. 750 f.:

Caesaris *\*illud \*opus*, voti pars altera vestra occidit

and

et magnum magni Caesaris *\*illud \*opus*

and

neque enim de Caesaris actis  
ullum maius opus, quam quod pater exstitit huius.

The simple facts are these: Ovid is immensely fond, for metrical reasons, of *opus*, which he employs more than one hundred times in pentameter closes alone. He is immensely fond also of the lightly moving *ille, illa*, which he uses in every part of his verse by the hundreds, and even of the obsolescent *illud*, which he uses by the scores. *Illud* is rare in the classical poets before Ovid, with the exception of Lucretius, i.e., 0 Tib., 0 Prop., 8 Cat., 6 Verg., 12 Hor., 29 Lucr.

<sup>1</sup> One translator well renders: "No form so fair adorned the age of gold, | No form so fair could spring from human mould." With the Ovidian phrase one may compare an expression used by the French statesman, Clemenceau, in New York: "Among the great works of the British—a little in spite of themselves—is the United States of America" (*New York Times*, Nov. 23, 1922).

Ovid, however, has resuscitated this form, and uses it freely<sup>1</sup> in every part of the verse, including the pentameter close, as *Trist.* iv. 10. 36, *illud onūs*; *ad Liv.* 176, *illud iter*; *A.A.* iii. 248, *illud edī*; cf. *Rem.* 301, *illud et illud habet*. \**Illud* \**opus* or \**illud* . . . \**opus* is a striking phrase which occurs only in Ovid and his "imitators," the *illud* being often unnecessary to the sense and almost meaningless in fact, but extremely convenient *metri causa*.<sup>2</sup> Its occurrence is as follows: 1 *Lygd.* (iv. 26—verse-close), 1 *Maec.* (150—verse-close), 2 *Moret.* (66, 69), 1 *Aetn.* (195, *illi operi*), 8 *Ov.* (*ad Liv.* 39; *Met.* vi. 129; *Trist.* i. 1. 68; i. 7. 29; ii. 328; iii. 14. 21; *Pont.* iii. 4. 28; *Fast.* i. 564). Four of Ovid's examples are in verse-closes.

The smallest and minutest details agree with the usage of Ovid. Thus the pronouns are usually omitted in Latin poetry, but we find them expressed in *Lygdamus*, precisely as is the case in the lively and familiar style of Ovid. Consider the following: \**huius* \**carminis*, i. 15: 4 *Ov.*, see page 365; †*huius* †*nominis*, i. 27: 6 *Ov.*, 1 *Hor.*, see page 370; \**haec* \**somnia*, iv. 95: 1 *Ov.* (*Met.* ix. 475); \**hoc* \**coniugium*, iv. 79: 1 *Ov.* (*Met.* x. 613); \**illud* \**opus*, iv. 26: 4 *V.A.*, 8 *Ov.*; †*hic* †*timor*, vi. 25: 2 *Ov.*, 1 *Prop.*,<sup>3</sup> \**haec* \**verba* \**dicere*, i. 22, see page 359; \**nostris* \**avis* i. 2: 1 *Ov.*, see page 368; \**nostrum* \**natalem*, v. 17: 1 *Ov.* (*Trist.* iii. 13. 2); †*cor* †*meum*, v. 12: 2 *Ov.*, 1 *Lucr.*, where nominative and accusative singular *cor* is very rare except in Ovid. The fourfold phrase \**perque* \**suos* \**iuravit* \**ocellos* (*oculos*), vi. 47, is found only in *Am.* iii. 3. 13; †*mea* †*membra* (not separated), v. 28, is found many times in Ovid and once in *Propertius* (iii. 16. 6).

It is no accident that the pretty and elegant booklet sent to *Neaera* on the festival of the *Matronalia* (*Lygd.* i. 9 ff.) and the unkempt, neglected book of the "Sorrows" (*Trist.* i. 1. 3 ff.), which the poet sends to his friends at Rome from his lonely place of exile, show the same vivid and picturesque powers of description, and both alike refer to the color or lustre (*color*, i. 18 and i. 1. 6) of the book, the sheets (*charta*) of papyrus, the title (*littera* or *titulus*), the bosses (*cornua*) and their projection at the two ends of the roll (\**geminae* \**frontes*),<sup>4</sup> the

<sup>1</sup> Fifty-seven times, according to *Crispinus*.

<sup>2</sup> *Middendorf, Elegiae in Maecenatem* (Marburg, 1912), p. 72; cf. also *Lillge, De elegiis in Maec.*, p. 29.

<sup>3</sup> *Her.* xiii. 92; xviii. 89; *Prop.* i. 19. 4.

<sup>4</sup> A favorite phrase of Ovid's, occurring also in *Fast.* i. 135.

smoothing with pumice stone (*pumex*), and the "hairs" or edges of the page which are to be left ragged and disheveled in the one case (i. 1. 12, *hirsutus sparsis comis*), and to be finished off neatly and daintily in the other (i. 10, *pumex et canas tondeat ante comas*); cf. here also Gruppe, *op. cit.*, page 129.

Surely we may conclude from this brief survey that of all the "imitators" and all the "sedulous apes" who have ever lived—*quot sunt quotque fuere . . . , quotque post aliis erunt in annis*—Lygdamus was the most unique and the most perfect, with the possible exception only of those other "imitators" who wrote the "Double Epistles," the *Consolatio*, the *Maecenas*, the *Priapea*, and the *Sappho*.

III. SUMMARY OF PHRASES COMMON ONLY TO OVID AND  
LYGDAMUS, ELEGY I

The visual method we employ below (p. 368) to exhibit the extent of the correspondences between Ovid and Lygdamus is easy for even a hasty reader to follow. It seems best, however, to bring together here in a compact form the twenty-five starred phrases from Elegy I which are common only to Ovid and Lygdamus. These phrases are as follows:

1. \**Martis* \**kalendae*
2. \**nostris* \**hic* \**avis*
2. \**nostris* \**avis*
3. f. \**pompa* \**perque* \**vias*
4. \**perque* \**vias* \**urbis*
4. \**per* . . \**vias* \**urbis*
12. \**indiket* \**nomen*
12. \**littera* \**facta* [4 Ov.]
13. \**geminas* \**frontes* [2 Ov.]
14. \**mittere* \**opus* [2 Ov.]
15. \**auctores* \**huius* \**carminis*
15. \**huius* \**mihi* \**carminis*
15. \**huius* \**carminis* [4 Ov.]
17. \**cultum* \**libellum*
18. \**sicut* \**erit* [10 Ov.]
19. \**mihi* \**referet* . . \**nostri*
19. \**illa* \**mihi* \**referet*
19. \**mutua* \**cura* [4 Ov.]
22. \**haec* \**dicite* \**verba*
22. \**dicite* \**verba* \**sono* [2 Ov.]
23. \**vir* . . . \**frater* [more than 6 Ov.]
25. \**teque* \**suis*

26. \*futura \*soror  
 26. \*coniunx . . . \*soror<sup>1</sup>  
 28. \*spem \*auferet

IV. PHRASES COMMON ONLY TO OVID AND LYGDAMUS,  
 ELEGIES III AND V

I have brought together in my former article<sup>2</sup> the twenty "Ovidian" phrases occurring in the first half of the third elegy.<sup>3</sup> It seems desirable to add here the remaining twenty-five phrases of the third elegy and the thirty-four phrases of the fifth elegy, so that they may be easily seen at a single glance:

III

21. \*non . . . \*curaeque \*levantur  
 21. \*mentes \*curaeque \*levantur  
 22. \*fortuna \*tempora (regit)  
 24. \*at \*sine \*te  
 24. \*regum \*munera  
 25. \*poterit \*mihi \*reddere  
 26. \*O \*felicem . . . \*quater  
 27. \*quaecumque \*voventur  
 28. \*meus \*deus  
 28. \*non \*meus  
 30. \*quas \*sustinet  
 30. \*sustinet \*orbis [different construction]  
 30. \*orbis \*opes [different construction]  
 31. f. \*liceat \*mihi . . . \*posse \*frui  
 31. \*liceat \*frui [2 Ov.]  
 31. \*coniuge \*frui  
 31. \*cara \*coniuge [3 Ov.]  
 33. \*adsis . . . \*faveas  
 33. \*adsis \*votis  
 33. \*faveas \*votis  
 33. \*timidis \*votis  
 34. \*Cypria, \*vecta  
 35. \*si \*fata \*negant  
 35. \*fata \*negant  
 36. \*sorores \*neunt

V

2. \*unda \*adeunda  
 3. \*sacris \*proxima  
 4. \*cum \*se \*remittit

<sup>1</sup> Where *soror* is not used in the literal meaning of "sister," but in that of "close relative" or "cousin."

<sup>2</sup> *Op. cit.*, p. 168.      <sup>3</sup> In v. 16 \*marmoreumque, 3 Ov., 2 V.A., was omitted.

4. *\*vere* *\*remittit*
6. *\*iuveni* *\*nocere*
6. *\*parce* *\*dea*
7. *\*temeranda* *\*sacra*
8. *\*sacra* *\*docere*
9. *\*pocula* *\*sucus* [A.A. ii. 335]
9. *\*pocula* *\*sucus* . . *\*dextera*
11. *\*templis* *\*admovimus*
11. *\*admovimus* *\*ignes* [3 Ov.]
12. *\*facta* *\*nefanda*
13. *\*insanae* *\*iurgia* (mentis)
14. *\*impia* *\*ora*
14. *\*solvimus* *\*ora* [6 Ov.]
15. *\*et* *\*nondum* [6 Ov.]
16. nec VENIT tardo CURVA SENECTA PEDE [repeated]
16. *\*curva* *\*senecta*
17. *\*natalem* *\*nostrum*
18. CUM CECIDIT FATO CONSUL UTERQUE PARI [repeated]
18. *\*cum* *\*cecidit*
18. *\*cecidit* *\*fato*
18. *\*cecidit* *\*consul*
18. *\*uterque* *\*pari* [2 Ov.]
19. QUID FRAUDARE IU VAT VITEM CRESCENTIBUS UVIS
20. et modo NATA mala VELLERE POMA MANU [repeated]
20. *\*modo* *\*nata*
20. *\*nata* *\*poma*
20. *\*vellere* *\*poma*
21. *\*quicumque* *\*tenetis* [2 Ov.]
21. f. *\*tenetis* *\*regna* [2 Ov.]
22. *\*tenetis* *\*tertia* *\*regna*
22. *\*tertia* *\*regna* [2 Ov.]
25. *\*pallebunt* *\*ora*
25. *\*rugosa* *\*senecta* [3 Ov.]
27. *\*vano* *\*terrear*
27. *\*aestu* *\*languent*
28. *\*ter* *\*quinos*
29. *\*celebrantur* *\*numina*
31. *\*vivite* . . . *\*vivite*
31. *\*memores* *\*vivite* *\*nostri*
32. *\*fuisse* *\*velint*
34. *\*pocula* *\*mixta*

V. METHOD EMPLOYED IN VISUAL EXHIBIT

In the text the asterisk and dagger are employed as already indicated (p. 358). If bold-faced type alone is used, the phrase is repeated in the later Ovid, but is found also in two other poets of our group. Italics are employed to indicate correspondences with the

Vergilian or the Tibullan Appendix. In the commentary the asterisk is used in a special way, and is prefixed to any very striking words or phrases.

#### VI. VISUAL EXHIBIT OF ELEGY I IN RELATION TO OVID

1. **\*Martis** Romani festae venere **\*kalendae** *Fast.* iii. 135; cf. [Tib.] iv. 2. 2. **\*festae** **\*kalendae** [Tib.] iv. 2. 21: **venere kalendae** *Fast.* i. 705.
2. exoriens **\*nostris** **\*hic** fuit annus **\*avis** *Trist.* ii. 472.
3. et vaga nunc certa discurrunt undique **\*pompa** | **perque** **\*vias** *Am.* iii. 13. 12.
4. **\*perque** **\*vias** **\*urbis** munera perque domos *Fast.* ii. 553; cf. *Fast.* iv. 186 (urbis per . . vias): †**perque** †**vias** *Fast.* ii. 221; Verg. *Aen.* ii. 364; cf. *Her.* x. 128 ( . . que per . . vias).
- 1-4. Cf. [Tib.] iv. 2. 2 tuis, Mars magne, kalendis; also cf. Hor. *Carm.* iii. 8. 1 Martiis kalendis. **\*nostris** **avis**: *avis* with a possessive pronoun is not found in our poets outside of Ovid. Ovid has three examples: *Am.* i. 8. 66 (a. tuos); *Trist.* ii. 472 (a. nostros); *Fast.* iii. 222 (a. suos). *discurro*, extremely rare in our poets: 3 Ov., 2 T.A., 1 V.A., 6 Verg., 0 al. **\*\*pompa** (Grecism), extremely rare outside of Ovid: 24 Ov., 1 Lygd., 1 V.A., 6 al. *per vias* (without *que*) three times in Ovid: *Am.* iii. 13. 12; A.A. iii. 709; *ad Liv.* 280. *perque vias* . . *perque domos* appears to be imitated from the famous lines of Verg. *Aen.* ii. 364 f. *Plurima perque vias sternuntur inertia passim | corpora perque domos et religiosa deorum.*
5. †**dicite**, †**Pierides**, quonam donetur honore *Fast.* ii. 269; vi. 799; Verg. *Ec.* viii. 63.
6. seu mea, †**seu** †**fallor**, cara Neaera tamen *Her.* v. 127 (*nisi*); cf. *Fast.* iv. 623 (*ni*); *Aetn.* 347 (*si*); Verg. *Aen.* v. 49 (*nisi*).
7. carmine formosae, pretio capiuntur avarae
8. gaudeat, ut digna est, **versibus illa meis** *Trist.* iv. 4. 31; v. 7. 26; *Pont.* ii. 2. 8; Verg. *Geor.* ii. 42; Prop. iii. 24. 4; Lucr. iii. 36.
- 5-8. *Pierides*, an Ovidian favorite: 18 Ov., 3 T.A., 1 V.A., 13 al. *digna est*: aphaeresis with *digna* is frequent in Ovid (*Am.* ii. 13. 22; A.A. iii. 766. etc.); v. Ganzenmüller, *Nux*, p. 26. *fallor* and *fallar*, 11 Ov., 1 Lygd., 3 V.A., 2 al. (*fallimur*, 6 Ov., 2 al.). *pretium*, an Ovidian favorite: 56 Ov., 1 Lygd., 1 V.A., 24 al. + 19 Hor.
9. lutea sed niveum involvat membrana libellum
10. pumex et canas †**tondeat** ante †**comas** *Her.* xi. 116; *Met.* xi. 47; Verg. *Geor.* iv. 137: **canas comas** A.A. iii. 75; Prop. ii. 18. 18; iv. 9. 52; Tib. i. 2. 92; i. 6. 82.
11. †**summaque** praetextat tenuis †**fastigia** chartae *Met.* ii. 3; Verg. *Aen.* i. 342, etc. (3 Verg.).
12. **\*indcet** ut **\*nomen** littera facta meum *Met.* iv. 686: **\*littera** **\*facta** *Her.* v. 2; *Trist.* i. 11. 2; iv. 1. 96; *Pont.* ii. 10. 4.
- 9-12. On the following descriptions see above. *luteus*, rare: 4 Ov., 1 Lygd., 2 V.A., 8 al. *involvere*, very rare except in Ovid and Vergil: 5 Ov., 1 Lygd., 1 V.A., 3 al. + 10 Verg. *membrana*, rare: 2 Ov., 1 Lygd., 7 al. **\*\*libellus** (diminutive), vss. 9 and 17, special favorite of Ovid: 53 Ov., 2 Lygd., 4 V.A., 18 al., 0 Verg., Tib., Lucr. *pumex*, rare: 9 Ov., 1 Lygd.,



- 10 *al.* Cf. [Tib.] ii. 3. 12 *intonsae comae*. \**charta* (Grecism), infrequent except in Ovid and Horace: 14 Ov., 6 T.A., 5 V.A., 8 *al.*+15 Hor.; see also Ehrengruber, *De Paneg.*, II, 34. 36. The *Thesaurus* correctly notes the word as rare in classical prose, but it is quite misleading about its use in Vergil and Tibullus. Cf. *Met.* i. 549 *tenuis liber. summaque*: 4 Ov. (*Am.* iii. 6. 6; *Met.* xi. 620; xiv. 50; *Fast.* i. 308), 2 Verg., 1 Prop., 0 *al. praetexo*, very rare: 4 Ov., 1 Lygd., 2 V.A., 2 *al.*+5 Verg. \*\**littera* (sing.), used only by Ovid for plural *litterae* (see p. 359). *indicare*, rare: 8 Ov., 1 Lygd., 8 *al.* Practically the same vocabulary of description used in Lygd. i. 9 ff. is to be found reversed in *Trist.* i. 1. 3 ff., where the poet describes the little book of the "Sorrows" which he sends to his friends at Rome from his lonely exile.<sup>1</sup>
13. atque inter \**geminas* pingantur cornua \**frontes* *Trist.* i. 1. 11 (same meaning); *Fast.* i. 5. 35: †*cornua* †*frontes* 10 Ov., 2 V.A., 2 Verg., 0 *al.*: A.A. i. 308; *Met.* iii. 139; xv. 133. 596; *Trist.* i. 1. 8; *Fast.* v. 606; vi. 197; *Hal.* 3; *Culex* 16; *Maec.* 116; Verg. *Ec.* vi. 51; *Geor.* iv. 499; cf. *Met.* ix. 688 (*fronti* | *cornua*) and *Met.* x. 222 (*cornu* | *frons*).
14. sic etenim comptum \**mittere* oportet \**opus* *Pont.* i. 1. 2; ii. 11. 2.
15. per vos, \**auctores* \**huius* mihi \**carminis*, oro *Fast.* vi. 709; *Met.* vii. 148 (*a. horum*, sc. *carminum*): \**auctores* \**mihi* *carminis* *Aetn.* 4: \**huius* \**mihi* \**carminis* *Fast.* v. 147: \**huius* \**carminis* *ibid.* iii. 724, 725; v. 147; vi. 709: †*auctores* †*carminis* *ibid.*; *Culex* 12; Tib. ii. 4. 13; cf. *Met.* vii. 149 (*a. horum*, sc. *carminum*); see note.
16. Castaliamque umbram Pieriosque lacus
- 13-16. *denim*: 3 Ov., 1 Lygd., 8 *al.*+26 Lucr. *oportet*, rare: 7 Ov., 1 Lygd., 2 V.A., 12 *al. auctores carminis*: *auctor* is here used in the sense of "inspirer" or "patron." Horace has the phrase twice, but in a different sense, with *auctor* equal to "writer" or "composer"; see *Thesaurus*, s.v. "auctor," p. 1202, l. 60. Cf. [Tib.] iv. 5. 7 *per te dulcissima furta . . rogo. Castalia* and *Castalius*, extremely rare: 2 Ov., 1 Lygd., 1 *Culex*, 3 *al. Pierius*, very rare: 4 Ov., 2 T.A., 2 V.A., 1 *Priap.*, 6 *al.*
17. ite domum cultumque illi donate libellum *Met.* xv. 458; *Fast.* v. 36, etc.; Tib. i. 5. 73; Hor. *Sat.* ii. 5. 37; *Epod.* xi. 20, etc.: \**cultumque* \**libellum* *Trist.* iii. 1. 4; cf. *ibid.* i. 1. 3 (*l. incultus*).
18. \**sicut* \**erit*: nullus inde defluat color 10 Ov.: *Her.* xxi. 25; *Am.* ii. 5. 45; *Met.* iii. 178; vi. 243, 657; xii. 205; xiii. 585; *Fast.* i. 424; vi. 332; cf. *Met.* v. 601 (*eram*): †*nullus* †*color* *Am.* ii. 11. 28; *Med.* 98; Hor. *Carm.* ii. 2. 1.
19. illa \**mihi* \**referet*, si \**nostri* mutua cura est | an minor *Trist.* iv. 3. 10: \**illa* \**mihi* \**referet* *Trist.* iii. 12. 49; cf. *Fast.* v. 278 (*restitit illa mihi*); cf. *Met.* ii. 35 (*i. refert*): \**mutua* \**cura* *ad Liv.* 302; *Met.* vii. 800; *Fast.* ii. 64; ii. 730: †*cura* †*minor* *Pont.* i. 2. 76; Verg. *Aen.* v. 803.
20. an minor, an toto †*pectore* †*deciderim* *Pont.* ii. 4. 24; Catull. lxxvi. 24.
- 17-20. *Trist.* ii. 1. 14 (\**liber* loquitur) erubui domino \**cultior* esse meo; cf. *Trist.* i. 1. 3 (the author addressing his book) vade, sed \**incultus*. \**sicut*, 22 Ov., 1 Lygd., 3 V.A., 5 *al.*+7 Lucr. *inde* in the pentameter close is a striking peculiarity of Ov.: 20 Ov., 1 *Priap.*, 1 Prop., 0 *al. Trist.* iv. 3. 10 sitque memor \**nostri* neque, \**referte* \**mihi*. *mihi* *referre*: 3 Ov., 2 Prop., 1 Hor.,

<sup>1</sup> See Gruppe, *op. cit.*, p. 129.

- 1 Verg., 0 *al. nostri cura*: Ovid avoids the spondaic form later and uses in its stead *cura mei* nine times, but he retains the apt verse-close *obli<sup>s</sup> via no<sup>s</sup> stri* (*Met.* viii. 45; *Trist.* i. 8. 11; v. 7. 29). The use of *cura* for *amor*, very frequent in Ovid, is well seen in *Her.* xiii. 166: *sit tibi cura mei, sit tibi cura tui*. *Pont.* ii. 4. 24 \**excidere haec credam \*pectore posse tuo. decidere*: The natural word in this passage is *excidere* as in *Pont.* ii. 4. 24, but Ovid uses *decidere* to avoid hiatus. Cf. also *Pont.* ii. 10. 8 *exciderit . . ne tibi cura mei. toto pectore*, very frequent in Ovid: 15 *Ov.*, 4 *Verg.*, 2 *Catull.*, 0 *al.*
21. *sed primum meritam larga donate salute*
22. *atque \*haec submisso \*dicite \*verba sono* *Met.* xiv. 717; [*Tib.*] iv. 5. 18; cf. *Lygd.* iv. 42 (*edidit h. v.*): \**dicite \*verba \*sono* *Her.* xiv. 52; *Met.* xii. 203: *verba sono* *Her.* xiv. 52; *Am.* ii. 6. 24; *Met.* xii. 203; xiv. 429; *Tib.* i. 8. 2; *Prop.* ii. 33. 10: *dicite verba* 17 *Ov.*, 5 *T.A.*, 2 *Verg.*, 2 *Tib.*, 0 *al.*
23. *haec tibi \*vir quondam, nunc \*frater casta Neaera* *Her.* iii. 52; iv. 35; viii. 1; viii. 28 (*bis*); *Fast.* vi. 28, etc.
24. *mittit et †accipias †munera parva rogat A.A.* iii. 466; iii. 657; *Met.* iv. 655; *Trist.* v. 9. 14; *Ciris* 219; *Prop.* i. 10. 12: *munera parva Paneg.* 7; *Pont.* iv. 8.35; *Catal.* iii\*. 9; *Verg. Aen.* vii. 243; *Hor. Carm.* i. 28. 3; (*munus parvum*) *Am.* ii. 15. 27; *A.A.* ii. 256; *Fast.* ii. 534.
- 21-24. *submissus* (adj.), very rare in our poets: 8 *Ov.*, 1 *Lygd.*, 1 *V.A.*, 3 *al.* Cf. *submissa voce ad Liv.* 289; *Met.* vii. 90; *Pont.* iv. 3. 42; *Ciris* 355 (*s. vocibus*). *Her.* xiv. 52 *exiguo \*dixi talia \*verba \*sono*; cf. *Met.* xiv. 429 *verba sono tenui. \*verba* (nom. and acc. plur.): 317 *Ov.*, 11 *T.A.*, 2 *V.A.*, 45 *Prop.*, 65 *al.* This enormous development in Ovid is due in part to the use in the pentameter close, which begins here in *Lygdamus. ea (quae) verba locutus* occurs elsewhere (*Verg. Aen.* viii. 404; *Catull.* lxvi. 29), and Ovid has also *haec ego verba loquar* (*Fast.* iii. 486), but *haec verba dicere* belongs to Ovid alone. *dicere verba*: e.g., *Her.* xi. 69; *Am.* i. 4. 19; *A.A.* i. 709, etc.; [*Tib.*] ii. 2. 1; ii. 5. 94; iv. 4. 14; iv. 5. 18; *Verg. Aen.* iv. 650; vi. 231; *Tib.* i. 3. 52; ii. 1. 73. v. 23 ff.: This epigrammatic play upon *soror, frater*, and *vir* runs through all Ovid's works: *Her.* viii. 28 et, si non esses \**vir* mihi, \**frater* eras. | \**vir*, precor, uxori, \**frater* succurre \**sorori*; *ibid.* 1 f. adloquor *Hermione nuper \*fratremque \*virumque*, | nunc \**fratrem*; nomen coniugis alter habet (a passage restored to the text by Vahlen and even by Ehwald); *Met.* iii. 265 sum *Iovis et soror et coniunx, certe \*soror*; *Fast.* vi. 27 \**fratre* magis, dubito, glorier anne \**viro*; cf. \**sororia* saltem oscula (*Met.* iv. 334; ix. 359; *Am.* ii. 5. 25). See also *Her.* xi. 23 f., 59 f., 126 f.; iii. 52; iv. 35; *Met.* i. 351 f.; ix. 466 f., 528 f., 569 f., etc.
25. \**teque \*suis iurat caram magis esse medullis* *Her.* xvi. 262 (*suo*).
26. sive sibi coniunx sive \**futura \*soror* *Trist.* i. 7. 18 (*fuisse soror*): \**coniunx*: sive \**soror* *Met.* i. 351 ("cousin," spoken by Deucalion to Pyrrha).
27. *sed potius coniunx: huius spem nominis illi* *Met.* ix. 599; *Ciris* 330; [*Tib.*] iv. 5. 15; *Prop.* ii. 24. 51; iv. 5. 43; *Lucr.* (five times) i. 942; iv. 17, etc.: †*huius †nominis ad Liv.* 2; *Met.* ii. 36; *Trist.* ii. 179; *Fast.* iv. 860; vi. 12; vi. 139; *Hor. Epist.* ii. 1. 234.
28. \**spem . . | \*auferet exstincto pallida Ditis aqua* *Her.* xxi. 187.
- 25-28. *teque*: chiefly in the verse beginning and more than 20 *Ov.*, 0 *Tib.* *Met.* i.

351 (Deucalion addresses Pyrrha) o \*soror, o \*coniunx, o femina sola superstes | quam commune mihi genus et patruelis origo | deinde torus iunxit. Here as in Lygd. i. 26 *soror* does not equal "sister" but rather "close relative" or "cousin." The contrast between *soror* and *coniunx* occurs elsewhere in our poets—e.g., Ov. *Met.* iii. 265; Verg. *Aen.* i. 46; Hor. *Carm.* iii. 3. 64—but outside of Ovid its use is restricted to Juno as wife and sister of Jove in the literal sense. *iuro* (spondaic), an Ovidian favorite: 80 Ov., 5 T.A., 3 V.A., 32 *al.* \**nominis* (gen. sing.), rare outside of Ovid: 35 Ov., 1 Lygd., 9 *al.*<sup>1</sup>

KENYON COLLEGE

<sup>1</sup>In continuation of the present article, a visual exhibit of the second elegy of Lygdamus will be published at an early date in the *Classical Quarterly*.

THE ETRUSCAN ORIGIN OF THE ROMAN  
ALPHABET AND THE NAMES  
OF THE LETTERS

By B. L. ULLMAN

The evidence, new and old, which Hammarström<sup>1</sup> has brought together makes it certain, to my mind, that the Latin alphabet and the Latin names of the letters are of Etruscan origin. This view is not so inconsistent as it seems with the older idea that the Romans borrowed their alphabet directly from Italian Greeks. The new view distinguishes an older from the later Etruscan alphabet and derives the Latin alphabet from the former. This older alphabet is identical with that which scholars formerly thought was Greek. The early abecedaria (Marsiliana, Formello, Caere) are Etruscan rather than Greek.

But even in Hammarström's opinion the Romans were forced at an early period to borrow some letters of the alphabet from the Greeks. In Etruscan there was no distinction between surds and sonants, and consequently the letters B, D, K, Q were not used. This fact furnishes one of the arguments for the Etruscan origin of the Roman alphabet, as it explains the early Roman use of the character C for both *c* and *g*. How, then, did the Romans come by B and D? Hammarström is forced to the conclusion that they borrowed them directly from the Greeks. But he does not explain satisfactorily why they did not adopt the Greek practice of using C only for *g* and expressing their *c* sound by K. While K and Q were given up in later Etruscan, they are found in early inscriptions, so that Hammarström has no difficulty in deriving these Latin letters from Etruscan. The letters O and X likewise are missing in Etruscan, so that again Hammarström is led to assume direct borrowing from the Greeks by the Romans. A strong objection to this view is that the Romans would probably have added the four new letters at the end of the alphabet, as they did later with Y and Z, instead of inserting them in their original places.

<sup>1</sup> "Beiträge zur Geschichte des etruskischen, lateinischen und griechischen Alphabets," *Acta Societatis Scientiarum Fennicae*, XLIX (1920), 2.

The facts that Hammarström presents can be interpreted without assuming any direct borrowing from the Greeks; in other words, it can be shown, I think, that the entire Latin alphabet as originally constituted was obtained from the Etruscans. As my proof of this statement is based in part on the Roman names of the letters of the alphabet, I shall discuss these first and shall endeavor to show that they are ultimately for the most part of Greek origin.

In taking over the alphabet from the Phoenicians, the Greeks memorized the names of the letters in the order in which they found them. These names served to identify, by their initial sounds, the values for which they stood. Thus the character  $\beta$  was identified as the letter *b* because of the initial sound of the word "beta." As the Etruscans (and Romans) preserved the order of letters found in the Greek alphabet, they too must have learned names which revealed the sounds of the letters. But in Etruscan and Latin there is no trace of the Greek names of the letters; on the contrary, we find in Latin a new system of names from which our systems in the modern languages are derived (*a, be, ce, de, etc.*). Hammarström argues convincingly that the Romans obtained this from the Etruscans. As to the origin of the new system several alternative explanations suggest themselves: either the Etruscans first learned the Greek names and then changed to the new method, or they made up the new names in taking over the alphabet, or they already found them in use among the Western Greeks from whom they borrowed the alphabet. The one thing that is certain is that some sort of name must have been in use from the beginning.

In the new (Roman) system three main principles may be distinguished: (1) The vowels are pronounced and written as such. (2) Most of the consonants have names consisting of the letters representing their sounds followed by *e* (*be, ce, etc.*). (3) Liquids and a few other letters are pronounced syllabically and have an *e* prefixed to their own symbols in writing (*el, em, etc.*).

This classification leaves *h(ha)*, *k(ka)*, *q(qu)*, *x(ix)* unaccounted for. In these there is one slight clue to help us choose among the alternatives just mentioned. In Latin and early Etruscan *Q* is used before *V*, *K* before *A*, *C* before other letters. All three, apparently, had the same sound. The restriction of *Q* in this way is already found

in Greek. As this cannot be coincidence, the Etruscans must have taken this restriction from the Greeks. The sounds of the letters, as I have said, were conveyed from one language to another by their names. The fact that kappa and koppa differ only in their second letters may have been enough to suggest to the Etruscans that Q be used only before O(V), but they would have been much more likely to see the point if the Greek names had already been shortened to *ka* and *ko*, along with the change of beta to *be*, etc. As the Etruscans had no *o* sound, the name *ko* eventually became *qu* (*ku*). The naming of *ka* in this way led to the false notion that it could be used only before *a*, a notion that persisted into late Roman times.

There is other evidence that the new names originated among the Greeks, or at least that the process started with them. Of the vowels, the name of *e* (later epsilon), derived from Semitic *he*, had by pure accident been reduced to its mere sound. The same may be true of *o* (later omicron), from the Semitic *ayin*, though here the dropping of the last letter of the name (*n*) cannot perhaps be easily attributed to accident. In other words, the reduction of *ayin* may have been due to deliberate imitation of *e*(psilon). At any rate, when the Greeks at an early period added *u* (later upsilon) to the alphabet, they invented a new name consisting merely of the vowel sound, in imitation of *e*(psilon) and *o*(micron). It was therefore an easy step to extend the principle to *a* and *i*.

Similarly with the consonants. In the earlier Greek there was one consonant, and only one, whose name consisted of its own sound plus the vowel *e*. That was *p* (*πεῖ*), from Semitic *pe*. When several new letters were added to the alphabet, their names were modeled after this: *φῑ*, *χῑ*, *ξῑ*, *ψῑ*.<sup>1</sup> Is it mere coincidence that *b*, *c*, and other letters were named in a similar way by the Romans? I think not. The renaming of these letters would naturally take place at a time when the name type *pe* was popular, i.e., when the letters *ph*, *ch*, *x*, *ps* were added to the alphabet. The prominence of *p* as the father, so to speak, of the new series would lead first to the renaming of its close relative, *b*, which happened to be the first consonant in the alphabet. The other

<sup>1</sup> See my article in *Classical Philology*, XXII (1927), 136. I take occasion to point out a *lapis calami* in this article: on p. 137, line 2, "labial" and "palatal" should of course be interchanged.



consonants would then follow suit. I have argued<sup>1</sup> that the added letters were invented by Western Greeks. The Etruscan-Roman alphabet was borrowed from a Western Greek alphabet. This explanation not only attributes the new names to the Western Greeks from whom the Etruscans obtained the alphabet but also accounts for the use of the vowel *e* rather than another vowel in the Roman names of most of the consonants.

Schulze<sup>2</sup> has shown that the earliest Roman names of the letters which the Roman grammarians called semi-vowels were not *ef*, *el*, etc., but *f*, *l*, etc. (i.e., the mere sound, without prosthetic vowel). Hammarström has made it probable that this earlier system was used by the Etruscans for *l*, *m*, *n*, *r*. This is merely an extension of the vowel naming system. The later use of a preceding vowel was for convenience; *e* was chosen probably under the influence of the other consonants. Why *f* and *s* were named (*e*)*f* and (*e*)*s* rather than *fe* and *se* is not entirely clear. One thing is certain: the name (*e*)*f* indicates a strong labiodental spirant, like English *f*. There is some evidence that it was a bilabial earlier.<sup>3</sup> The corresponding Etruscan letter seems not to have been a spirant.<sup>4</sup> The Greek digamma never had the sound of *f*. Probably, therefore, the Romans changed the name from *ve* to *ef* after they had changed the sound. The sensitiveness of the Romans to the independence of the sound *s* (which led to the name (*e*)*s* instead of *se*) is shown by the use of a prosthetic vowel before "impure" *s* in Vulgar Latin.<sup>5</sup> But it is impossible to tell whether we owe the name *es* or merely its preservation to this Roman feeling.

The vowel *i* in the name *ix* is late, according to Schulze, and supplanted an earlier *ex* under the influence of Greek *xi*. The use of *ex* instead of *xe* may be due in part to the analogy of *s* (*x* = *ks*) but chiefly to the fact that no Latin word began with *x* and consequently the Romans may have had difficulty (as we do) in pronouncing *xe*. The change from *xe* to *ex* may therefore have been made by the Romans.

<sup>1</sup> *Loc. cit.*

<sup>2</sup> *Sitzungsber. d. preuss. Akad. d. Wiss.* (phil.-hist. Classe, 1904), pp. 760 ff.

<sup>3</sup> Lindsay, *Latin Language*, p. 99.

<sup>4</sup> Hammarström, *op. cit.*, p. 33.

<sup>5</sup> Cf. Schuchardt, *Der Vokalismus des Vulgarlateins*, II, 364, and Lindsay, *Latin Language*, p. 102, who suggests that Quintilian's *circa s litteram deliciae* may refer to an "on-glide."

One name is left entirely unexplained, *ha*.<sup>1</sup> One naturally concludes that it must bear some relation to *ka*, but what that relation is I cannot suggest, unless we assume that the name *ha* is late Roman, which is possible. In that case the influence of *ka* could be explained through the fact that *h* between vowels was pronounced and written as *ch* or *c* (*michi*, *mici*).

To return to our chief concern: the Etruscan origin of the Roman letters B, D, O, X. In the early Etruscan abecedaria we find letters for which the Etruscans had no need (B, D, K, O, Q, X). We find two of them (K, Q) in occasional Etruscan inscriptions of the early period. Certainly the names, and hence the sounds, of these letters must have been preserved. Granting that the Etruscans had no use for B, for example, there is no reason to believe that in taking over this character along with the rest of the alphabet they failed to take over the name (and hence the value) of that letter.<sup>2</sup> To be sure, in the course of time the pronunciation of the unused letter would tend to degenerate, i.e., to be made identical with that of P, but as long as it continued to be written in abecedaria there would be an attempt, especially on the part of those who knew Greek, to keep the original pronunciation in saying the *abc*'s. The numerous Greek words which came into Etruscan are proof enough that many Etruscans knew Greek. No doubt, in the speech of educated Etruscans, words borrowed from Greek retained their Greek pronunciation for some time, just as in English the French pronunciation of such words as *garage* and *chauffeur* persists in the speech of the cultured. As long as the Etruscans used B, D, etc., in their abecedaria they must have had a distinguishing pronunciation for them.

The Etruscans apparently borrowed their alphabet from the Greeks in the eighth century. A much earlier period is scarcely possible, for one thing because the added letters of the Greek alphabet

<sup>1</sup> Hammarström's elaborate explanation (*op. cit.*, pp. 34-42) is not convincing.

<sup>2</sup> The same is true of such characters as *samekh*, found in the Marsiliana abecedarium. Cf. also the maintenance of zeta in the Latin alphabet until the letter G was invented; that its position was maintained is made certain by the fact that G took its place. Grenier is quite wrong (*Mélanges d'archéologie et d'histoire*, XLI [1924], 1 ff.) in arguing that the Etruscan alphabet is not of Chalcidian origin because the Marsiliana abecedarium contains *samekh* and other letters not found in Chalcidian inscriptions. One must not compare abecedaria, which contain a traditional and theoretical alphabet, with inscriptions.

are found in Etruscan, and these can hardly antedate the ninth century. To be sure, they might have been added later, but there is no evidence to this effect. The Romans probably received the alphabet from the Etruscans in the seventh century—the earliest Latin inscription belongs to that century or the early sixth. In other words, the Romans probably obtained the alphabet from the Etruscans not more than a century after the latter adopted it, before the names and values of the letters B, D, O, X had disappeared from the *abc*'s. But, it will be asked, what of the combination of *c* and *g* sound in the character C? The facts about the name take care of this situation. In the case of B and P, D and T, the very likeness of the names (*be*, *pe*, etc.) makes it necessary to be careful to distinguish the sounds of the letters in order to get a differentiation. In the case of C, K, Q (*ce*, *ka*, *qu*) the situation is quite different. As we have seen, a differentiation of usage had arisen as a result of the difference in names, based on the following vowel. Therefore greater carelessness in distinguishing the actual consonantal difference between C (gamma) and K was natural. It may be asked why the Etruscans preserved the original sound of O in the *abc*'s long enough to pass it on to the Romans, whereas they changed the name of Q from *ko* to *ku* before the Roman adoption. The answer again is that in the case of O as a letter of the alphabet a conscious effort was made to differentiate it from V, whereas in the name of Q the exact sound of the second letter was unimportant.

The foregoing arguments are based on the following principles:

1. When a language takes over an alphabet from another language and preserves the order of letters, this is proof positive that the alphabet was introduced from abecedaria accompanied by oral sounding of the letters or their names, not from documents.
2. Abecedaria are conservative in preserving letters used rarely or not at all in writing the language.
3. The preservation of an unused letter in such circumstances presupposes the preservation of the original pronunciation of the name, even if the sound does not exist in that language; or if the pronunciation changes in the course of time, it will still be differentiated in some way from other letters of the alphabet. It would be intolerable for two letters to exist which not only were sounded alike but also had the same names.

## ASTERIS AND DULICHIMUM: A REPLY

FRANK BREWSTER

Mr. Shewan, in a recent paper,<sup>1</sup> has advanced further reasons against my identification of Asteris with Arkudi and Same with Leucas. As he frankly admits, on page 199, that he does not understand at least a part of my argument, it seems worth while to endeavor to make my position clearer.

The theory upon which my several papers<sup>2</sup> were based is that Homer himself had never visited the Ionian islands but had obtained his geographical details as to Ithaca from some pre-existing source. The general theory, stated by Allen<sup>3</sup> and *The Cambridge Ancient History*<sup>4</sup> is that Homer composed the two epics in Asia Minor from existing sources. There is no direct evidence that he ever visited the islands. There is, however, indirect evidence that he never saw them. The passage in  $\epsilon$  21-26 as commonly rendered seems to indicate that Homer conceived of Ithaca as the farthest west of the four islands; and it also contains two other inaccurate statements. As our text stands, Dulichium, Same, and Zacynthus are apparently included in a group of many islands very near one to the other, and this group is directly located far to the east of Ithaca. Both of these statements are inaccurate if Thiaki is Ithaca. Leucas, Cephalonias, and Zacynthus do not form a part of any group of many islands very near each other, and not one of them is far to the eastward of Thiaki. If, however, we analyze the passage it becomes obvious that the difficulties are all due to the presence of line 24, containing the three other names. If we omit this line, the passage becomes a perfectly good and accurate description of Thiaki from "the point of view of a voyager along the trade

<sup>1</sup> A. Shewan, "Asteris and Dulichium," *Classical Philology*, XXI, 193 ff. For convenience I shall refer to this and his two preceding papers, also published in *Classical Philology*, by number of volume and page only.

<sup>2</sup> "Ithaca," *Harvard Studies*, XXXI, 125 ff.; "Asteris," *ibid.*, XXXIII, 65 ff.; "Ithaca, Dulichium, Same and Wooded Zacynthus," *ibid.*, XXXVI, 43 ff. For convenience these will be subsequently referred to by volume and page only.

<sup>3</sup> Thomas W. Allen, *The Homeric Catalogue of Ships* (1921), p. 21.

<sup>4</sup> II, 507.

route from Pylos." This was the theory I presented in my first paper, and it was approved by Mr. Shewan in *Classical Philology*, XIX, 298.

Taken by itself, line 24 is another accurate description of Thiaki. Thiaki is one of a group of four large islands, and there is today a general agreement that they represent the four islands Homer names. Differences of opinion relate to identifications merely.

If Homer had ever personally visited the Ionian islands, he must have been aware that this description was inaccurate. If he had never seen the islands, the mistake of joining these two independent descriptions was perfectly natural, as it is practically impossible to obtain a correct impression of the relative position of localities from description merely. If Homer had no more information of Ithaca than what we learn from the *Odyssey*, he could not possibly have formed any correct idea of its relative position in regard to the other islands. That Homer deliberately joined two descriptions of Ithaca in a way that he must have known produced an inaccurate result did not seem to me probable. I therefore adopted the theory that Homer had never visited the islands.

A necessary consequence of this theory was that I had to seek an adequate source for the passage in Book ix. I regarded it as a nautical description and gave my reasons for so thinking in XXXI, 153 ff. Mr. Shewan thinks me wrong, but does admit that it is "from the point of view of a voyager along the trade route from Pylos." In either case it implies a description taken from the story of a voyage from Pylos northward. Now we know that a story of the Argo was older than Homer. It is a fair inference from its long-continued popularity that it was a favorite. We know that commerce between the Aegean and the West Mediterranean existed before Homer.<sup>1</sup> It seems probable that the story of the Argo reflected the adventures of these sailors.<sup>2</sup> If so, the voyage followed the trade route, north from Greece, and may well have been the source from which Homer took this description.

The story of Telemachus' return contains no geographic details except the names of certain places passed as the boat cruised north-

<sup>1</sup> *The Cambridge Ancient History*, p. 459; Gustave Glotz, *The Aegean Civilization*, Book II, chap. v, and esp. pp. 220 ff.

<sup>2</sup> *The Aegean Civilization*, pp. 392, 393.

ward along the coast, and then the course headed for Oxia. As this is in accordance with my theory of the trade route, these details also may have come from the same story of the Argo.

Neither of these stories necessarily implies that Homer took either from the old story of the Argo; but they do imply some sea-story founded on actual experience. Mr. Shewan seems to have made the same inference. In XXI, 202, he writes as to the geographical references to Ithaca: "For myself, after reading the discussion, I cannot say there is a single matter of which the poet could not have become cognizant in the course of a voyage from the Peloponnesus, a short stay on Ithaca, and a sail round the island." This is in effect a statement that such details as the poet gives us could only come from a real observer. Mr. Shewan makes Homer that witness. This I could not do on the theory on which I was working. Lacking Homer, the most probable witnesses were the sea-captains who navigated these waters. My theory that their knowledge and adventures were embodied in the story of the Argo which Homer knew may not prove finally to be the best. But it seems entirely possible, and it seems also the most likely story of which we know to contain such descriptions.

Mr. Shewan writes (XXI, 201) that Bérard's suggestion that the Odyssean geography is taken from an old Phoenician periplus "was the part of Bérard's work that found least favor." Whether this refers merely to the Phoenician idea, or the periplus idea, or the general principle of Bérard's book is not clear. Probably the latter, as the others are merely details. How this is today, I do not know; but it should not be forgotten that when Dörpfeld announced that Leucas was one of the Homeric islands, the idea met much opposition. Today it seems to be generally accepted.

To turn now to some details of Mr. Shewan's paper which seem to me important. The first one that needs consideration is the reiteration of his opinion that the formation on Arkudi does not constitute real havens. This is of real importance because, if correct, it eliminates a physical fact of great value as evidence. On this question, Seymour writes: "This island [Arkudi] also has on the east a small peninsula, with harbors on either side of its isthmus, so near each other and so much alike that they may be called twins."<sup>1</sup>

<sup>1</sup> Thomas Day Seymour, *Life in the Homeric Age*, pp. 71 and 72.



Manatt writes:

How Dörpfeld's heart throbbed when he first approached it and discovered the double harbor not unlike—to compare small things with great—the twin havens of Mitylene and still sheltering on occasion the small craft that do business in these waters. With *carte blanche* to invent an Asteris, one could hardly have hit it better.<sup>1</sup>

Dr. Leaf writes:

Now midway in the channel between Leukas and Thiaki there actually lies an island, now called Arkudhi, exactly answering to what Homer says. It is truly "not large," for it is only 2 miles long by 1 broad. But it does, at its southeastern extremity, contain a little double harbor, formed by a low rocky peninsula jutting far enough into the sea to form on either side of it twin coves in which a low vessel, with its mast down, might lie unnoticed, right upon the course which must needs be taken by a vessel sailing from Pylos to Leukas. Its hill runs up to a height of 400 ft. above the sea, enough to serve as the "windy outlook" where the ambushed suitors kept all day relays of sentinels to watch for the approach of Telemachus. If this is mere coincidence, and not description from knowledge, surely coincidence has done its utmost to flout us.<sup>2</sup>

In "Leukas-Ithaca," Mr. Shewan wrote:

So far the description favors Daskalio, but the rest of it—*λιμένες δ' ἐν ναύλοχαι αὐτῇ ἀμφίδυμοι*—raises much difficulty. The Leukadists point with triumph to "twin havens" in Arkoudi, and photographs of these are given by Goessler and Seymour (*Life in the Homeric Age*). This is perhaps the best piece of evidence in favour of Leukas. Bérard in fact declares (*op. cit.*, II, 483) that it is the "one solid argument" in Dörpfeld's whole Leukas-Ithaka case.<sup>3</sup>

Again, on page 233, Mr. Shewan writes:

The words *λιμένες ἀμφίδυμοι* are more important and are variously translated. . . . But it cannot be denied that the meaning may be only "twin havens" or "a pair of havens," and the pair found in Arkoudi may correspond to what the poet had in his mind.

Again, on the same page:

One thing is certain, the Ithakists cannot shew a real *λιμὴν* on Daskalio at the present day. . . . Part of Daskalio may have been destroyed, or much of its shore may have been worn away by wind and tide. But it is only a possibility. The fact remains that to-day, as in Strabo's time, Daskalio offers οὐδ' ἀγκυροβόλιον εὐφύες.

<sup>1</sup> James Irving Manatt, *Aegean Days* (1914), p. 384.

<sup>2</sup> Walter Leaf, *Homer and History* (1915), p. 152.

<sup>3</sup> *Journal of Hellenic Studies*, XXXIV (1914), 232.

These passages are not cited for the purpose of showing that Mr. Shewan is now contradicting himself. He thought then, as he thinks now, that this formation could not be regarded as real havens. The citations, however, do show that he took a more liberal view at that time of the possibilities of a different opinion.

Mr. Shewan's idea of the shape of these havens seems founded on a misconception. Thus, in the same article, he says: "The bay to the left can hardly be so called, for the shore seems to stretch away in a straight line." It does look so in the photograph, but von Marées' chart shows a deep indentation of the main line of the shore by this line. Bérard's chart also shows indentations at what seems to be the right spot. The little isthmus is really at about right angles to the main body of the island; and von Marées' incident of a vessel running in here in a southerly storm<sup>1</sup> shows that it was efficient as a harbor. To answer fully this objection would require repetition of what I wrote in XXXVI, 84 f., which should not be done here.

The only grounds I saw for Mr. Shewan's argument in XIX, 312 f., that Krokyleia was "Arkoudi" were sundry opinions, some of which were to the effect that the two names referred to were districts of Ithaca and others to the effect that they were islands. This does not seem to prove much. Then we are told that there is "evidence" from Pliny that it was an island. Mr. Allen gives these opinions including Pliny more fully, and he apparently regards them as inconclusive, for he writes: "Besides these parishes of Ithaca or islands."<sup>2</sup> Even if Krokyleia were proved an island, that does not prove it to be Arkudi. The only item in the whole passage that bears directly on this point is the suggestion that the name Arkudi is derived from the name Krokyleia, and that, as I noted, Professor Chatzidakis, whom he referred to, did not admit.

As a matter of fact, Leake's opinion,<sup>3</sup> that these names were names of districts in Ithaca, always seemed to me the most probable.

As to Aphales Bay, Bérard says it is unsuitable as a harbor.<sup>4</sup>

The question whether the suitors could have seen Telemachus' ship from Daskalio, as she approached the south end of Thiaki, is

<sup>1</sup> XXXVI, 87.

<sup>2</sup> *Op. cit.*, p. 91.

<sup>3</sup> William M. Leake, *Travels in Northern Greece*, III, 49.

<sup>4</sup> *Les Phéniciennes et l'Odyssée*, II, 459.

purely one of mathematics. It is important as bearing on the improbability of Mr. Shewan's theory that the course for Ochia was for the purpose of avoiding being seen by the suitors. If Homer had sailed round Ithaca as Mr. Shewan conjectures, he must have had a fair idea of the actual distances.

In regard to the eastern route, there seem to me to be two distinct questions. If Homer framed the story of the return from his own knowledge, then the question would be how a boat would ordinarily sail going from Pylos to Ithaca on the bay of Polis. On this, my conclusion was that we did not have sufficient evidence to say with reasonable certainty which side of Thiaki she would sail, and therefore the story could not be used as evidence against Arkudi.<sup>1</sup> The second question is (on my theory that Homer took the geographic facts from an account of the trade route), where the trade route ran. So far as Homer described this voyage, it follows my theory of the trade route.

The goddess' advice to keep away from the islands fits the trade route, for it is exactly what the route does. It does not seem to me that it does fit Mr. Shewan's theory. Mr. Shewan criticizes my expression "pitch dark." What I had in mind was that it was full night, which is what Homer says. Of course you can see for some distance at night, but obviously not so far as to see a boat off the shore of Elis from the neighborhood of Daskalio. Surely it is not discrediting Homer to assume that if he had sailed round Ithaca he would be conscious of such an obvious fact.

In regard to the trade route, the distance from Cape Trepito, the most westerly point of the Peloponnesus, to a point off the north end of Cephalonia by way of the Ithaca Channel scales about fifty geographic miles according to the large-scale United States chart. The distance from the same point via Ochia and by Arkudi scales about fifty-eight geographic miles. This difference is not very large. On the other hand, we have: (1) the probability in the fair-weather season that the seamen could sail all the way and avoid a long row through the channel;<sup>2</sup> (2) the fact that squalls in narrow channels between high mountains

<sup>1</sup> Dr. Leaf came to the same conclusion (*Homer and History Appendix*, p. 351). Allen does the same (*op. cit.*, p. 93); but his theory that it was all fiction does not preclude the inference that the poet took his geographic facts from an existing source. The details are too definite to be produced by the imagination only.

<sup>2</sup> The channel scales about twelve miles long.

are really more dangerous than squalls out in the open; (3) the fact that so far as Homer describes Telemachus' course, it follows this route; (4) the fact that the Argo coming south from Coreyra passes by the same important landmark, the Echinades; (5) the fact that Mentès, when he deviates from his trade route to visit Odysseus, enters Frikes harbor on the east side of Thiaki. This looks as if this was the port traders used, and, if so, the ordinary trade route was east of Thiaki; (6) we find on this route an island which corresponds to Homer's description of Asteris, and one which, from Bérard's description of the custom of seamen of that time, would appear of real use. The trade route must have taken one of these two courses. The evidence seems to me to be distinctly in favor of the eastern route.

In XXI, 200, Mr. Shewan wonders "if some one will ever be tempted, in dependence on modern handbooks, to analyze the hurly-burly raised by Poseidon in € 292 ff." He will find modern analogies given by Bérard in I, 481 ff., and also Glotz, *The Aegean Civilization*, page 4.

On the same page, Mr. Shewan imputes to me views about Polis which I have never entertained or expressed. Obviously conditions that might keep out traders would also tend to keep out pirates, and would, therefore, attract inhabitants.

On the same page he asserts that in my view the crew took the ship from the haven now called S. Andrea round the east side of Thiaki. As a matter of fact, I pointed out that there are two ports at the south end of Thiaki, one S. Andrea, the other Ligia on the east side of the island near its southerly end; that the poet does not say which port Telemachus landed in. Either suits the narrative. From Ligia the return might have been either way so far as distance is concerned. There would apparently have been a better chance for a fair wind by the east side of Thiaki.

Exactly what Mr. Shewan is trying to prove by his comments on the passage from the *Argonautica* is not apparent to me. Vessels going south on the eastern trade route, I have advocated, would pass exactly the localities named and in exactly the order named. Mr. Shewan tries to make it out a jumble, but he only does so by creating facts which are not in the poem. If the poor bard pleads he "does but sing because he must," this would seem to imply that he was following the tradition which would point back to a Homeric trade route.

Mr. Shewan's argument about the *πρὸς Ἡλίδος* passage, φ 347 f., appears to be this. The two suitors named in the context were Ithacans. Therefore, Telemachus' remark was addressed to the leaders. Amphinomus, the leader of the Dulichiums, was the best of the suitors and was, therefore, not included. Therefore Dulichium was not included in the *νήσοι*. Leucas does not lie *πρὸς Ἡλίδος*. Therefore Leucas is Dulichium. Telemachus begins his speech by saying, "No man of all the Achaeans has a better right than I." This is sweeping, and to assume that the vast majority of the wooers were excluded in his subsequent speech seems revolutionary. As all the wooers were described as rulers in α 245 ff., π 122 ff., and τ 131 ff., they would naturally be included in the expression *ὄσσοι . . . κοιρανέουσιν*. Even if the reference to Elis excludes Leucas, that does not prove it is Dulichium. The expression *οὐθ' ὄσσοι . . . οὐθ' ὄσσοι* must include all the wooers from the other three islands. Therefore Dulichium cannot be excluded on account of the merits of its leader. Therefore Leucas may be either Same or Dulichium. Either would agree with the text.

As to the Thesprotian tale, my argument was that, if Cephalonia were Dulichium, a vessel bound for a port on its eastern side would have to pass suitable ports on Ithaca to land at, and we should not even have to assume any deviation; but that if Leucas were Dulichium we should have to assume a deviation. Why I should be charged with "the belief, which others have shared, that the shipmen went out of their way in order to have supper at Thiaki" seems astonishing. In XIX, 49, Mr. Shewan claims that the *κακὴ βουλὴ* of the crew "makes the narrative consistent with the theory that S. Maura is the Dulichium of Homer." If this means merely that the story is not necessarily inconsistent with the identification of Dulichium with Leucas, I think this should be admitted; but not more than this. If the story fairly implies a deviation, which I doubt, it might just as well have occurred after the stop on Ithaca as before. If the deviation had already occurred, there is no evidence that the landing on Ithaca was for the purpose of selling Odysseus, and if the crew were then bound for another port to do this, they might just as well have returned to Cephalonia as to Leucas. In any aspect, therefore, the story cannot be regarded as evidence that Leucas was Dulichium.

In regard to the name Same, Strabo's definition would apparently make it applicable to any high place. If so, it is hardly distinctive.

Of course, modern conditions do not prove ancient conditions; but when you find the populations of three ancient localities<sup>1</sup> have about the same relative proportions as the modern populations of the same localities, you have a coincidence which requires an explanation. It would be quite remarkable if Leucas in Homer's time had relatively as much larger a population than Cephalonia as Cephalonia now has than Leucas. Whereas there is nothing improbable in the assumption that the two islands were equally well developed in Homer's time. In that case, their relative populations would be proportionate to their respective areas, and might well have had the same relative proportions as now.

I quite agree with Mr. Shewan that there is no evidence that Dulichium was part of the realm of Odysseus. As a matter of fact, such evidence as we have would seem to limit his kingdom to Ithaca itself.<sup>2</sup> I do not, however, admit his statement that there is no evidence that Leucas is Same. It seems to me, on the contrary, such evidence is positively found in the "Catalogue of Ships." In B 631 ff. is enumerated the contingent led by Odysseus. As part of this contingent, we find (l. 635) οἳ τ' ἡπειρον ἔχων ἡδ' ἀντιπέραι' ἐνέμεντο. Murray (Loeb ed.) translates this line: "and held the main land and dwelt on the shores over against the isles." ἀντιπέραια is plural and would include all such shores. There are, in fact, only two which fit: the part of Acarnania over against Leucas and the part of Elis opposite Zacynthus. If Odysseus led the men from both these shores, this part of Acarnania could not have formed part of Dulichium. It would be very improbable if this part of Acarnania did not belong to the realm of Leucas; and if Leucas is Same, the statement of the "Catalogue" is consistent. We are also told that Laertes when he was lord of the Cephallenians conquered Nericus (ω 376 ff.), and Thucydides mentions a Nericus in the immediate neighborhood of Leucas, which I think with Dörpfeld was probably in Acarnania.<sup>3</sup> It would be natural, therefore, if this district went under the leadership of Odysseus. On the other hand, there is no mention of any men from any mainland in

<sup>1</sup> Allen, *op. cit.*, p. 90, regards the number of suitors as indicative of population.

<sup>2</sup> See on this α- 386 f. and 401 f., χ 52.

<sup>3</sup> I omit my reasons for this, as it would lengthen this paper too much to insert them here.



the contingent under Meges. This seems surprising if, as Allen and Shewan suggest, Acarnania formed an important part of Dulichium.

Mr. Shewan apparently does not object very strenuously to my suggestion that the leaderships mentioned in the "Catalogue" do not necessarily indicate political units, but he quite misunderstands my allusion to the Echinades. Homer calls them (B 625 f.) *ιεράων νήσων*. Why were they holy? I had supposed it was true, as Mr. Shewan states, that there was no famous sanctuary there. Indeed I know of no mention of any temple there. But Tarn says that "one of the qualities of a hieron was that you might not carry war into it or sack it."<sup>1</sup> This would be very valuable to traders. Hence my suggestion. The epithet surely requires an explanation. Mine may not be the best, but Mr. Shewan does not offer us any.

I am at a loss to understand Mr. Shewan's line of thought in discussing my suggestion as to the change of names. I had supposed it was an admitted fact that no trace of its Homeric name remained on Leucas when we come to the time when history begins. Also that it was an admitted fact that the names of Same and Dulichium were borne by two places in Cephalonia, and are still there. If Leucas was one of the Homeric islands, it must have once had the name of Dulichium or Same. Names, as a rule, do not change without a cause. Certainly the foregoing facts justify the inference of a dispossession of inhabitants, by invasion, and a transfer of the place-name to Cephalonia. Of course, they do not prove which name was transferred. There is no evidence that the name Ithaca was ever displaced; but surely Mr. Shewan does not mean to contend that there was as much probability of a recollection of a rather insignificant name like Same as of the much more famous name of Ithaca.

Mr. Shewan says the "towering Monte Negro" is "a few miles away" from Leucas.<sup>2</sup> The distance, according to the chart, between the south end of Leucas and Monte Negro is about 30 miles, and to Mount Stavrota on Leucas about 40 miles. The *Mediterranean Pilot* gives the total length of Cephalonia as 27½ miles, and of Leucas barely 19. The westerly shore of Leucas is one long mountain side with

<sup>1</sup> W. W. Tarn, "The Political Standing of Delos," *Journal of Hellenic Studies*, XLIV (1924), 141 f. and 142. That this sanctity could be extended to places not temples, see cases cited on pp. 142 and 143.

<sup>2</sup> XXI, 205.

Mount Stavrota 3,700 feet, Mount Elias 3,300 feet, and Mount Meganoro 3,300 feet. The mountain ridge of Cephalonia stands well back from the shore. Monte Negro is 5,218 feet. Other heights range from 3,212 feet down.

If *Dulichium* means "Long Island," which is doubtful, it would seem as if this name suited Cephalonia best, as it is the longest of the four islands. On the other hand, *Leucas*, with its long wall of mountains, with their peaks comparing favorably with anything on Cephalonia except Monte Negro, might very well be called high. It would be my impression that either name would suit either island sufficiently well to make it impossible to draw any profitable distinction between them on account of the names.

The question of the true meaning of *πορθμός* seems to me of real importance, and I trust that no scholar, who is interested in the question of the identity of *Asteris*, will form any final opinion without first considering the passages from Pindar to Strabo which I have cited in XXXIII, 72 ff., and XXXVI, 69 ff. If any other concept than that which I have suggested better suited the fundamental idea of this word, as used in these various passages, Mr. Shewan should certainly have been able to show it. So far as I can make out, he does not even attempt this, and his only argument is that it had a different meaning in the time of Homer. This was not Seymour's opinion.<sup>1</sup> He cites passages from the classic writers to prove that *πορθμῆς* in *v* 187 is not limited in meaning to our word "ferryman," as Mr. Shewan now thinks. Seymour's statement is cited in full in XXXIII, 70. Homer defines the vocation of his *πορθμῆς* in the following words: (*v* 187 f.) *οἳ τε καὶ ἄλλους ἀνθρώπους πέμπουσιν, ὅτις σφέας εἰσαφίκηται*. He uses exactly the same words to define the vocation of the Phaeacians in *π* 227 f. *μ* 40 is not a third occurrence. In this passage the essential words are different. The fact that the Sirens treated the public differently from the Phaeacians and the *πορθμῆς* does not help us in defining the last word. It was my impression from the foregoing that *πορθμός* and *πορθμεύς* were terms used in trade or commerce and, therefore, had the same sense shown as the underlying meaning by classic usage.

I do not know who the "ancients" are to whom Mr. Shewan refers,

<sup>1</sup> *Op. cit.*, p. 71 n.

but if they are the lexicographers I should suppose that classic usage was the better guide.

As to *μεσσηγύς*, Palmer, Murray (Loeb ed.), and Butcher and Lang all render it by our word "midway." I take it, therefore, that this is its primary meaning.

Dunbar's *Concordance* shows that *εἶναι* is used only twice in the *Odyssey* in the sense of "anchor stones": once in the description of the harbor in the island off the land of the Cyclops (ι 137), where the boats were in fact beached, and next in ο 498 when Telemachus landed on Ithaca, and they only stopped for breakfast.

*πρυμνήσια*, according to the same *Concordance*, occurs eleven times. Five of these—ι 178, ι 562, λ 637, μ 145, and ο 548—are the usual formula for a start. I infer this because, as to ι 178, we have just been told in ι 137 that anchor stones and stern cables were not needed; and in ι 148 that the ship had been simply beached.<sup>1</sup> The other instances are β 418, where the ship had just been drawn down and launched; ι 137 noted above; ο 286, where the ship had probably been hauled out (see γ 365); ο 498, when Telemachus stops for breakfast; ο 552, part of the same episode, and μ 32, where the ship had been previously beached (see μ 5). In the other formula cases, in ι 562, the ship had been beached (see ι 546); in λ 637, the ship had been beached (see λ 20); in μ 145, here the ship had been beached (see μ 5); ο 547 is part of the Telemachus episode.

So far as the characteristics of a good harbor are concerned, it would not seem to make an important difference whether the boats were merely beached or drawn up high and dry.

In regard to the use of anchor stones, Merry and Riddell, in their note on δ 785, suggest that if the cable was not fastened to the stone but run through a loop at the stone and the loose end carried ashore, the boat could then be pulled out from the shore and drawn back by the stern cable as desired. This is a simple device, and practically the same as that now in common use for mooring small boats which you wish to reach from land, but which cannot be kept hauled up for various reasons. It is not only an excellent explanation of the passage in question, but gives a good explanation also of why Odysseus had to swim to reach the shore in ξ 350 f. In the last instance the crew ap-

<sup>1</sup> Merry and Riddell, *Odyssey I*, p. 369, have a note to the same effect.

parently were all on shore, and how the boat could be lying in water deep enough to require swimming has always been a puzzle to me. The foregoing suggestion, however, would explain it.

Whether my other suggestions are as futile as Mr. Shewan thinks does not seem very important; but certainly nautical expressions should be interpreted in the light of nautical customs then in use.

It is a source of genuine regret to me to find myself at variance with Mr. Shewan, with whose ideas generally I am in hearty accord. Naturally, with his deserved reputation, I feel as if there was more probability of error on my part than on his. It seemed to me, however, that the only course to pursue was to present my ideas fairly before scholars, in the hope that someone else might be sufficiently interested to analyze our respective arguments and ascertain where the error has crept in, or what has produced the difference of opinion between us.

HARVARD UNIVERSITY

## THE MANUSCRIPTS OF THE *FLORILEGIUM* OF THE LETTERS OF SYMMACHUS

BY JAMES E. DUNLAP

A Latin manuscript, now the property of the University of Michigan Library, and known as MS 154, consists of 144 leaves, and contains the *De consolazione* of Boethius and a collection of the letters of Symmachus. The leaves of the manuscript are of parchment, and in their present condition measure 10.5 by 14.6 centimeters. They were once somewhat larger than this, for on many leaves the decoration at the top, the bottom, or the outer margin has been encroached upon in the process of trimming. On each page, the space occupied by the text measures 6.6-6.8 centimeters in width, and 10.1 centimeters from top to bottom. This space is bounded by double lines, and is ruled, thirty lines to the page. The lines contain from 33 to 41 characters each, with an average of 38.

The manuscript is all written in the same hand. It is a rather small minuscule, with lightly shaded strokes, and little adornment, save that the tops of the tall letters in the first line of each page project upward into the margin, and are marked by modest flourishes. Abbreviations are numerous, and as there is little or no differentiation in the vertical strokes employed to form many of the small letters, it would often be quite impossible to interpret with any assurance certain groups of characters if taken out of their context.

The illumination of the manuscript is purely decorative, and is found only in conjunction with adorned capital letters. On the recto of the first two leaves of the *De consolazione* appear leaf and flower scrolls in blue, green, brownish-purple, and gold, which fill the margins at the right, the top, and the bottom. Elsewhere, capital letters are accompanied by an adornment of the right margin in fine red lines, usually wavy or serrate, and terminating in sweeping, reflex curves. The initial letters of each epistle are capitals of medium size, occupying a space of two to three lines in height, and approximately square. The letters themselves are blue, generally inclosed within squares of light red lines, while the spaces within the letters are filled by curving

red lines in various irregular patterns. The text of the letters of Symmachus begins with a page similarly decorated with red and blue, but in a somewhat more elaborate style.

The handwriting displays certain characteristics usually assigned to the fourteenth century, and the style of ornamentation is conformable to that employed between the years 1300 and 1400. The date of the manuscript therefore falls somewhere within this period.

The contents of the manuscript are:

	Folio
<i>De consolatione</i> of Boethius.....	1r- 67v
Letters of Symmachus.....	68r-121v
Index to <i>De consolatione</i> .....	122r-141v
Blank.....	142r-144v*

\* 142r and 144v are ruled.

In the second division of the manuscript there appear, in the order in which they are mentioned, the following letters, according to the division and numbering of Seeck's<sup>1</sup> edition:

I, 1; 5; 6; 14; 23; 25; 28; 31; 32; 34; 36; 37; 38; 43; 45; 46; 47; 56; 60; 61; 67; 74; 75; 76; 77; 79; 80; 82; 83

II, 16

III, 1; 2; 3; 4; 5; 6; 7; 8; 9; 10; 11 [as two letters, the second beginning with *de mea aegritudine*]; 13; 15; 17; 18; 21; 22; 23; 24; 25; 26; 29; 30; 31; 35; 36; 37; 40

II, 51 and III, 42 [without division]; II, 40

III, 43; 45; 44 [to *salii canunt*]; 51; 46; 53; 48; 44 [continued from *at augures*]; 54; 64; 65; 66; 67; 61; 63; 56; 57; 58; 69; 70; 71; 72; 73; 74; 77; 78; 79; 80; 81; 82; 83; 86; 90; 91

IV, 1

I, 84; 86; 88; 90 and 91 [without division]; 92; 93; 96; 97; 99; 100; 105; 107 and II, 1 [without division]

II, 3; 6; 8; 22; 27; 29; 32; 35; 37; 39; 41; 42; 48; 49 and 50 [without division]; 56; 58; 63; 68; 70; 71; 73; 79; 80; 87; 88; 89; 90; 91; 64

IV, 4; 11; 15; 16; 17; 20; 25; 26; 28; 30; 32; 35; 38; 39; 40 and 41 [without division]; 47; 49 and 50 [without division]; 51; 54; 56 and 57 [without division]; 58 [as two letters, the second beginning with *gemina ante*]; 59; 60 and 63 [without division]; 66; 67; 69; 72

V, 3; 4; 5; 8; 13; 19 and 21 [without division]; 23; 24; 29; 30; 34; 36; 38; 41; 44; 45; 46; 47; 49; 50; 51; 53; 55; 57; 59; 60; 61; 65; 67; 68; 70; 71; 73; 75; 77; 78 and 80 [without division]; 79; 84; 85; 89; 91; 92; 96

VI, 3; 13; 18; 22 [to *graviora esse referenda*]; 28; 31; 45; 47; 55; 60; 61; 65; 72; 73; 74; 78; 79; 80

VII, 2; 3; 9; 11; 16; 19; 21; 22 and 25 [from *repensa tamen*, without division];

<sup>1</sup> "Q. Aurelii Symmachi quae supersunt," *Monumenta Germaniae historica, auctores antiquissimi*, Tomus VI, Berlin, 1883.



not joined

33; 44; 47 and 49 [without division]; 51; 52; 53 and 54 [without division]; 56; 60 [as two letters, the second beginning with an *ego adversum*]; 61 and 64 [without division]; 66; 67; 71; 72; 73; 78; 80; 85; 88; 92; 93; 94; 98; 99

*Relatio 3*

It is obvious that in this manuscript we have another copy of the *Florilegium* of the letters of Symmachus, described by Seeck in the Preface to his edition.<sup>1</sup> It must be noted at the outset, however, that the text of the Michigan codex differs from all others of its kind in two important features. First, while the manuscript is itself intact, the text of the *Florilegium* which it contains is incomplete, lacking approximately the last third of the usual contents. Second, there are striking peculiarities in the arrangement of the contents of the manuscript. For example, the letters from i. 84 to ii. 64 are displaced from their proper position and are inserted after iv. 1, with the notable exception of the short letter ii. 16, which did not suffer the transposition, although it would normally be found within the displaced portion of the text.

It is also to be noted that whereas, in the usual text of the *Florilegium*, the continuity of Book iv is interrupted by *Relatio 11*, *Ep. ix. 142*, and a part of *x. 2*, which have been intruded into the text following iv. 54, the continuity of Book iv is unbroken in the Michigan manuscript, which does not contain the inserted portions at all.

Lastly, at the close of the Michigan text of the letters, there has been added the famous *Relatio 3*, which does not appear in the common text of the *Florilegium*.

No adequate explanation of these peculiarities of the Michigan manuscript is possible at the present time, but their existence is enough to suggest that the Michigan tradition is not the same as that of other *Florilegium* manuscripts.

Seeck's edition affords but little material for the comparison and study of the manuscripts of the *Florilegium*. Seeck had no very high opinion of these manuscripts, and after giving an account of the two most important codices of the letters, P and V, he proceeds as follows:

Codicibus, quos descripsimus, multo peior est ea familia, quae florilegium [F] ex litteris Symmachianis continens in exemplaribus plus minus abbreviatis aut truncatis per omnes paene bibliothecas maiores dispersa reperitur. . . . Haec codicum familia omni interpolationum genere incredibilem in modum scatet neque ullius usus est, ubi Parisini et Palatini subsidii uti licet. Lec-

<sup>1</sup> Pp. xxviii ff.

tiones igitur eius in septem libris prioribus in apparatus criticum non recepi, nisi ut eius indoles fontibus melioribus collocatis perspiciatur. cui consilio ea sufficere putabam, quae iam in totius classis archetypo fuisse videbantur, quare singulorum codicum nunc superstitum errores omisi.<sup>1</sup>

Nowhere does Seeck state what manuscripts he employed in determining the reading of the *Florilegium* archetype. In the critical apparatus of Books viii–x, where the readings of the *Florilegium* are of the greatest importance, he records the variants from three sources. These are: F<sup>1</sup>, an early printed edition which, he states, reproduces with absolute fidelity a manuscript no longer extant; F<sup>2</sup>, a manuscript of the Bibliothèque nationale in Paris, which he collated; and F<sup>3</sup>, a Vatican manuscript collated by Otto Hirschfeld. If Seeck considered these three sources sufficient in passages where the *Florilegium* forms the sole basis of the text, it is quite unlikely that he employed other sources when reconstructing the *Florilegium* archetype for the purpose of mere illustration. It is therefore reasonable to assume that this archetype reading, F, has been obtained by comparison of F<sup>1</sup>, F<sup>2</sup>, and F<sup>3</sup> only, and of these manuscripts Seeck judged that F<sup>2</sup> and F<sup>3</sup> were inferior to F<sup>1</sup>, and that they were less closely related to it than to each other.<sup>2</sup>

The inadequacy of Seeck's critical apparatus makes necessary an entirely new comparison of the *Florilegium* manuscripts in attempting to establish the relationship of the Michigan manuscript to others of its class. In making this comparison it will be convenient to employ the following symbols:

P *codex Parisinus* 8623, saec. IX

V *codex Vaticanus Palatinus* 1576, saec. XI<sup>3</sup>

F Seeck's reconstruction of the archetype of the *Florilegium* manuscripts

F<sup>2</sup> *codex Parisinus latinus* 8559, saec. XII/XIII

F<sup>3</sup> *codex Vaticanus Reginae* 1575, saec. XIII

C Corpus Christi College [Cambridge] MS 202, saec. XIII

Mich. University of Michigan MS 154

A Text of Ausonius, which preserves a few letters of Symmachus

Photostat reproductions of F<sup>2</sup>, F<sup>3</sup>, and C were employed; P, V, F, and A were taken from the apparatus of Seeck's edition; Mich. itself was available for study. Seeck's F<sup>1</sup> has not been accessible, but its readings

<sup>1</sup> Pp. xxviii f.

<sup>2</sup> Pp. xxix f.

<sup>3</sup> Seeck regarded these manuscripts so highly as to say (p. xxxii) that if they were complete they would be quite sufficient for the establishment of a reliable text. It will be seen that his practice does not conform to his statement.

may generally be assumed to be those of F<sup>2</sup> in the cases where F, F<sup>2</sup> agree against F<sup>3</sup> in the following tables.

The first fact to become apparent from the comparison of these manuscripts is that the manuscripts of the *Florilegium* do not form a homogeneous group, a fact which is demonstrated by the following variants. It is to be understood, in comparing these readings, that where there is no mention of F, Seeck judged that the *Florilegium* archetype was in agreement with the text which he printed. Furthermore, the absence of reference to F<sup>2</sup> in the first three passages cited is explained by the fact that that manuscript is defective at its beginning. The figures refer to page and line of Seeck's edition.

TABLE I

10.	3	<i>nostra ora</i> ] CF <sup>3</sup> Mich. ✓	<i>ora nostra</i> F
	4	<i>amissus</i> CF <sup>3</sup> Mich. ✓	<i>amisos</i> F
14.	13	<i>ut cetera morum tuorum</i> ]	<i>om.</i> CF <sup>3</sup> Mich. ✓
15.	27	<i>gnarus</i> ] FF <sup>2</sup>	<i>non ignarus</i> CF <sup>3</sup> Mich. ✓
17.	5	<i>servare secretum</i> ] F <sup>2</sup>	<i>secretum servare</i> CF <sup>3</sup> Mich. ✓
	23	<i>tuam legi</i> ] F <sup>2</sup>	<i>legi tuam</i> CF <sup>3</sup> Mich. ✓
18.	10	<i>tibi</i> ] FF <sup>2</sup>	<i>om.</i> CF <sup>3</sup> Mich. ✓
	11	<i>parentem et amicum</i> ] F <sup>2</sup>	<i>parente et amico</i> CF <sup>3</sup> Mich. ✓
22.	19	<i>societatem</i> FF <sup>2</sup>	<i>societatem</i> CF <sup>3</sup> Mich. ✓
23.	10 12	<i>summa est</i> ] F <sup>2</sup>	<i>est summa</i> CF <sup>3</sup> Mich. ✓
24.	3	<i>amoris</i> FF <sup>2</sup>	<i>in minoribus</i> CF <sup>3</sup> Mich. ✓
27.	14	<i>merito</i> ] F <sup>2</sup>	<i>merita enim</i> CF <sup>3</sup> Mich. ✓
	15	<i>librata</i> ] F <sup>2</sup>	<i>libata</i> CF <sup>3</sup> Mich. ✓
28.	22	<i>usu</i> FF <sup>2</sup>	<i>iussu</i> CF <sup>3</sup> Mich. ✓
	30	<i>carissimos</i> ] F <sup>2</sup>	<i>kmos</i> CF <sup>3</sup> Mich. — <i>sondat</i>
	31	<i>vigilias</i> ] F <sup>2</sup>	<i>vigilantias</i> CF <sup>3</sup> Mich. ✓
29.	3	<i>exposcimus</i> ] F <sup>2</sup>	<i>poscimus</i> CF <sup>3</sup> Mich. ✓
30.	21	<i>non ausim</i> FF <sup>2</sup>	—— CF <sup>3</sup> Mich. ✓
33.	12	<i>magnifacio</i> ] FF <sup>2</sup> ✓	<i>magnifico</i> CF <sup>3</sup> Mich. ✓
37.	3	<i>convenior</i> ] F <sup>2</sup>	<i>praevenior</i> CF <sup>3</sup> Mich. ✓
	21	<i>poculo</i> ] F <sup>2</sup>	<i>flumine</i> CF <sup>3</sup> Mich. ✓

It will be seen from the foregoing examples that there are at least two distinct groups of *Florilegium* manuscripts, and that C, F<sup>3</sup>, and Mich. are more closely related to one another than they are to F<sup>2</sup> and the other manuscripts which form the basis of the F readings given by Seeck.

Seeck seems to have been quite unaware that the *Florilegium* manuscripts were divided into sharply distinguished groups. This is

not strange if, as has been assumed, he employed only  $F^1$ ,  $F^2$ , and  $F^3$  in reconstructing his  $F$  archetype, for the deviations of any one of these manuscripts from the common reading of the other two would very naturally be regarded as meaningless vagaries of that one codex. It is somewhat surprising, however, to observe that Seeck considered  $F^2$  and  $F^3$  more closely related to one another than to  $F^1$ ,<sup>1</sup> for if the evidence of Table I is valid, it certainly indicates that  $F^2$  and  $F^3$  do not belong to the same branch of the  $F$  tradition. Whether  $F^2$  is actually more closely related to  $F^1$  than to  $F^3$ , as the same table seems to suggest, cannot definitely be decided until  $F^1$  is available for study.

On further examination it becomes evident that Mich. is in some degree independent of the tradition of  $C$ ,  $F^3$ . Table II makes this sufficiently obvious.

TABLE II

2.	27	<i>optionis</i> ] VMich.	<i>optionis</i> CF <sup>3</sup>
5.	28	<i>religiosam magis esse</i> ] VMich. ✓	<i>magis esse religiosam</i> FCF <sup>3</sup>
6.	1	<i>in nostram venit</i> ] VMich. ✓	<i>venit in nostram</i> FCF <sup>3</sup>
10.	1	<i>iuuisti</i> ] VMich. ✓ <i>iuuasti</i> FC	<i>inuisisti</i> F <sup>3</sup>
	20	<i>offensi spargas</i> ] A	<i>offensi</i> Mich. ✓ <i>offensis parcis</i> CF <sup>3</sup>
17.	8	<i>ingratis</i> ] A	<i>gratis</i> Mich. ✓ <i>ingratus</i> VFCE <sup>3</sup>
	24	<i>ubi</i> ] VMich.	<i>tibi</i> C <i>cum</i> F <sup>3</sup>
18.	9	<i>tibi verus</i> ] V	<i>tibi nexus</i> Mich. ✓ <i>tui itineris</i> CF <sup>3</sup>
20.	3	<i>flavescere</i> ] V	<i>flatescere</i> Mich. ✓ <i>falcescere</i> FF <sup>3</sup> <i>falcessere</i> C
	8	<i>spectatissime</i> ] VMich.	<i>expectatissime</i> FCF <sup>3</sup>
24.	7	<i>residem</i> ] V	<i>resedem</i> Mich. ✓ <i>residere</i> FCF <sup>3</sup>
29.	3	<i>et</i> ] PVF <sup>2</sup> Mich.	<i>om.</i> CF <sup>3</sup>
33.	10	<i>officium est</i> ] PVMich. ✓	<i>est officium</i> F <sup>3</sup> <i>est indicium</i> C
	11	<i>postulatis</i> ] PMich. ✓	<i>postulantis</i> VFCE <sup>3</sup>
34.	12	<i>philosophi</i> ] PVMich.	<i>philosophiae</i> CF <sup>3</sup>
35.	6	<i>sollemnis</i> ] PVMich. ✓ <i>solemnis</i> Mich.	<i>sollemnus</i> CF <sup>3</sup>
	10	<i>imago versatur</i> ] PVMich. ✓	<i>versatur imago</i> CF <sup>3</sup>
	13	<i>etiam</i> ] PVMich. ✓	<i>et</i> CF <sup>3</sup>
	25	<i>referre</i> ] PVMich. ✓	<i>deferre</i> CF <sup>3</sup>
37.	30	<i>scribam</i> ] PVMich. ✓	<i>om.</i> CF <sup>3</sup>

It will be noticed in the foregoing examples that in those cases in which Mich., while departing from the reading of  $C$ ,  $F^3$ , is not without other support, it is quite frequently in accord with  $P$  and  $V$ , the manuscripts which seem to preserve, better than any others, the original form of the letters. This occasional agreement with the better manu-

<sup>1</sup> P. xxix.

scripts may indicate that an attempt was made to improve one of the predecessors of Mich. by bringing it into conformity with the better tradition. If such a recension was made, it can be assigned with the greatest probability to an early period in the history of the text. This conclusion is based on the fact that there are very many discrepancies between Mich. and the other manuscripts of the *Florilegium* which can only be explained as errors, and the number of these errors is so large that it suggests repeated copying, which, in turn, presupposes an interval of some length between the time of the assumed recension and the date of Mich. If this hypothesis is correct, it is impossible to avoid the conclusion that although C, F<sup>a</sup>, and Mich. belong to the same general tradition, Mich. represents, in some features, an early form of the P, V tradition, which may even be more correct than the actual readings of P and V.

Seeck's estimate of the value of the *Florilegium* manuscripts has been quoted, but they are not quite so worthless or unimportant as his words might lead one to believe. The present text of the letters of Symmachus is based very largely on P, V, and the *Florilegium*. There are, moreover, 66 letters in the *Florilegium* which are found neither in P nor in V; there are 112 letters in both P and the *Florilegium*, but not in V; there are 27 letters in both V and the *Florilegium*, but not in P. Obviously, the importance of the *Florilegium* in reference to the text of these 205 letters cannot be overlooked. Furthermore, though Seeck has said that the *Florilegium* is of no use where the evidence of P and V is available, he has himself taken into his text, in direct opposition to the evidence of P and V, or of one of them in the absence of the other, no less than twenty readings in the first twenty-five pages of his edition. When it is considered that the *Florilegium* contains only about half of the letters on these pages, the number of good readings which it has supplied assumes some importance.

It should be observed, further, that in the *Florilegium* manuscripts there can be found corroboration for a number of readings which Seeck has accepted into his text, but has printed as mere conjectures by early scholars. In the following examples the conjectured readings are followed by the names of their sponsors. The symbols indicate manuscript authority which has not been given by Seeck.

TABLE III

6.	2	<i>voluptatem</i> ] Lypsius, Mich.	✓ h k
22.	17	<i>cognatio</i> ] Iuretus, F <sup>3</sup>	✓ j v w
36.	14	<i>accedere</i> ] Iuretus, F <sup>3</sup>	✓ h o n p e v
56.	10	<i>novam</i> ] Lypsius, Mich.	
84.	6	<i>voluptate</i> ] Iuretus, Mich.	
112.	11	<i>probabitur</i> ] Lypsius, C	

md h k s v w  
In commenting on the beginning of *Ep. i. 97* (p. 39) Seeck says: "praecedenti epistulae cohaeret in PVF, divisit Iuretus." The division between the letters exists in F<sup>2</sup>, F<sup>3</sup>, and Mich.; in F<sup>2</sup> it is eight words out of its true place, but in F<sup>3</sup> and Mich. it occurs at the proper point.

Thus far, in estimating the value of the *Florilegium* manuscripts, only those readings have been considered which give support to Seeck's text, yet there are to be found in these manuscripts quite a number of variant readings which deserve consideration. At the present time any judgment of these matters would be premature, for much of the manuscript evidence is not as yet available. Nevertheless, the two examples which follow may be of some interest.

*Ep. iii. 65* ends thus: "simulque deprecor, ut adfectionem, quam mihi et praesenti dependere et absenti dignatus es polliceri, litterarum munere, quotiens usus tulerit, non graveris augere." Instead of *augere*, C has *arguere*.

*Ep. vi, 78*, written to Symmachus' daughter and her husband on the occasion of the former's birthday, begins with the sentence: "Aevum maneat hic dies, qui te nobis filiam dedit." In his critical notes Seeck gives the reading of the *Florilegium* archetype as *cum maneat*. In his Introduction<sup>1</sup> he gives *cum* as the reading of F<sup>1</sup>, and *in perpetuum* as the reading of F<sup>2</sup> and F<sup>3</sup>. At this point Mich. has the unique reading *Faustus in aevum*.

In conclusion, it may be said that the *Florilegium* manuscripts appear to be worthy of much more serious consideration than Seeck gave them. They require further study with a view to determining their relationship and reappraising their value, and the results of this new appraisal, together with the additional evidence which the Michigan manuscript is yielding, may make clear the necessity of a new edition of the letters of Symmachus.

UNIVERSITY OF MICHIGAN

<sup>1</sup> P. xxix.

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## THUCYDIDES AND CRATIPPUS

BY WILLIAM K. PRENTICE

There is a passage in Marcellinus' biography of Thucydides (secs. 30-33) about which there has been a great deal of learned discussion. It is not likely that any suggestion about any part of it can be made now which has not already been made by someone. Yet no convincing solution of all the apparent difficulties has been proposed by anyone, and few have agreed on any single interpretation of the passage as a whole.

The passage would not be important, and perhaps not very interesting, if it did not contain certain opinions of Cratippus and Zopyrus, who may have lived in the time of Thucydides himself. Dionysius of Halicarnassus says<sup>1</sup> that Cratippus was a contemporary of Thucydides (*συνακμάσας αὐτῷ*). If that is so, then Cratippus' testimony about Thucydides is much more valuable than all the arguments and deductions of later writers about him; consequently, Thucydides died in Thrace, probably by a sudden and violent death, and he was not buried among the "Cimonian monuments" at Athens. That would be interesting, and not without importance in the history of the literature of the ancient Greeks, for the light it would throw on the private life of the greatest historian of them all. But some of the best scholars have believed it quite impossible that Cratippus was a contemporary of Thucydides, and so have thought it necessary to emend the statement of Dionysius of Halicarnassus or to conclude that that uninspired author was mistaken again.

The question of the date of Cratippus has acquired a new importance since the publication of the Hellenica Oxyrhynchia in 1908. It seems certain that these fragments belong to a history written between 356 and 346 B.C.<sup>2</sup> Some have assigned the work to Cratippus. But if Cratippus lived and wrote in the Hellenistic age, he could not have been the author in question. Personally I believe that Professor

<sup>1</sup> *De Thucydidis caractere*, chap. xvi.

<sup>2</sup> B. G. Grenfell and A. S. Hunt, *The Oxyrhynchus Papyri*, Part V (1908), pp. 122 and 134; Eduard Meyer, *Theopomp's Hellenika* (1909), p. 120. Cf. K. Fuhr, *Berl. Philol. Woch.*, Vol. XXVIII (1908), Sp. 197 l., and J. H. Lipsius, *Cratippi Hell. Fragg. Oxyrh.* (1916), p. 3.

Eduard Meyer has shown that, as far as we can judge at present, the Oxyrhynchus fragments belong to the Hellenica of Theopompus. But if Cratippus was a younger contemporary of Thucydides, one of the chief arguments against the assignment of the Hellenica Oxyrhynchia to him is refuted.

The decision as to the date of Cratippus rests upon the interpretation of four passages in the ancient literature, and upon the deductions which may properly be made from them. For my own part I am inclined, in such matters, to adopt Pindar's motto, τὸ δὲ φυᾷ κράτιστον ἅπαν ("The natural way is best").<sup>1</sup> There would never have been so much confusion and disagreement on this subject if modern scholars, in their eagerness to know more about the ancient world, had not read into the statements of the ancients something more than the actual words contain.

The most important of the four passages referred to is the one already mentioned, in Marcellinus' *Vita Thucydidis*, sections 30-33:

§ 30. Οἱ μὲν οὖν ἐκεῖ λέγουσιν αὐτὸν ἀποθανεῖν ἐνθα καὶ διέτριβε φυγὰς ὢν, καὶ φέρουσι μαρτύριον τοῦ μὴ κεῖσθαι τὸ σῶμα ἐπὶ τῆς Ἀττικῆς. § 31. Ἰκρίον γὰρ ἐπὶ τοῦ τάφου κεῖσθαι, τοῦ κενотаφίου δὲ τοῦτο γνώρισμα εἶναι ἐπιχώριον καὶ νόμιμον Ἀττικὸν τῶν ἐπὶ τοιαύτῃ δυστυχίᾳ τετελευτηκότων καὶ μὴ ἐν Ἀθήναις ταφέντων. § 32. Δίδυμος δ' ἐν Ἀθήναις ἀπὸ τῆς φυγῆς ἐλθόντα βιαίῳ θανάτῳ φησὶν ἀποθανεῖν τοῦτο δὲ φησι Ζώπυρον ἱστορεῖν. τοὺς γὰρ Ἀθηναίους κάθοδον δεδωκέναι τοῖς φυγάσι πλὴν τῶν Πεισιστρατιδῶν μετὰ τὴν ἦτταν τὴν ἐν Σικελίᾳ· ἦκοντα οὖν αὐτὸν ἀποθανεῖν βίᾳ, καὶ τεθῆναι ἐν τοῖς Κιμωνίοις μνήμασι. καὶ καταγινώσκειν εἰήθειαν ἔφη τῶν νομιζόντων αὐτὸν ἐκτὸς μὲν τετελευτηκέναι, ἐπὶ γῆς δὲ τῆς Ἀττικῆς τεθῆναι· ἡ γὰρ οὐκ ἂν ἐτέθη ἐν τοῖς πατρώοις μνήμασι ἢ κλέβδην τεθεῖς οὐκ ἂν ἔτυχεν οὔτε στήλης οὔτε ἐπιγράμματος, ἢ τῷ τάφῳ προσκειμένη τοῦ συγγραφῆως μηνύει τοῦνομα. ἀλλὰ δῆλον ὅτι κάθοδος ἐδόθη τοῖς φεύγουσιν, ὥς καὶ Φιλόχορος λέγει καὶ Δημήτριος ἐν τοῖς ἄρχουσιν. § 33. ἐγὼ δὲ Ζώπυρον ληρεῖν νομίζω λέγοντα τοῦτον ἐν Θράκῃ τετελευτηκέναι, κἂν ἀληθεύειν νομίσῃ Κράτιππος αὐτόν. τὸ δ' ἐν Ἰταλίᾳ Τίμαιον αὐτὸν καὶ ἄλλους λέγειν κεῖσθαι μὴ καὶ σφόδρα καταγέλαστον ᾗ:

Some, then, say that he [Thucydides] died there where he also lived while an exile, and they bring evidence of his body not lying in Attica. For [they say] that an ikrion was upon the grave, and that this was the local and customary Attic mark of the cenotaph of those who died in such misfortune and were not buried at Athens. But Didymus says that *having returned from exile*

<sup>1</sup> *Olymp. ix.* 100.

he died at Athens, by a violent death. And this, he says, Zopyrus records. For [he says] the Athenians granted a return to the exiles, excepting the Peisistratidae, after the defeat in Sicily: so he returned and died by violence, and was buried among the Cimonian monuments. And he said he accused of folly those who consider that he [Thucydides] died abroad, but was buried in Attic soil; for either he would not have been buried among the family monuments, or, buried secretly, he would not have had either the stele or the inscription which [sc. the stele], placed on the tomb, reveals the name of the historian. But evidently a return was granted to the exiles, as both Philochorus says, and Demetrius in "The Archons." And I consider that Zopyrus was talking nonsense when he said that this [Thucydides] died in Thrace, even if Cratippus may consider that he speaks the truth. That Timaeus and others say he was buried in Italy is not far from very ridiculous.

Of course wherever, as in this instance, an author (Marcellinus) quotes from another (Didymus), who in turn is quoting from several others, it is very difficult to determine satisfactorily what was the testimony of each, and how much of the original sources was modified by the interpretations, and perhaps misunderstandings, of the later writers. Undoubtedly the principal source for the passage quoted above was the learned Didymus. But it is not possible to decide with certainty whether Marcellinus derived all the statements here, at least as far as their content is concerned, from Didymus, or makes direct use of some of the other authors and sometimes even expresses conclusions of his own. I have put in italics what I think Marcellinus probably found in Didymus. It is possible that the whole passage was derived from that indefatigable compiler, either directly or, as Petersen and Unger<sup>1</sup> thought, through Antyllus, whoever he was. Personally, however, I believe that at least the last two sentences, beginning with the words "And I consider," are Marcellinus' own.<sup>2</sup> This use of the personal pronoun "I" recalls the first paragraph of the biography, in which plainly Marcellinus himself is speaking personally to his pupils.

A more important difficulty lies in the fact that two statements apparently assigned to Zopyrus are contradictory, namely, that Thucydides died at Athens and that he died in Thrace. Various explanations have been offered, including some emendations of the text: the

<sup>1</sup> *Jahrb. f. Class. Philol.*, CXXXIII (1886), 103.

<sup>2</sup> So also Otto Gilbert, *Philologus*, XXXVIII (1879), 262-64.

most natural seems to have occurred only to Ritter and to Gilbert,<sup>1</sup> namely, that Zopyrus did not really contradict himself, and therefore cannot have said quite all that Didymus puts in the sentence "Having returned from exile, etc." Evidently Zopyrus did say that Thucydides died in Thrace. His other statement, to which reference is made, can have been only that Thucydides died a violent death. If that is the case, then the beginning of the first actual quotation from Didymus should be read: "But Didymus says that [Thucydides] having returned from exile died at Athens, by a violent death—and this [namely, that Thucydides died by violence] he says Zopyrus records." The statement, therefore, that Thucydides died a violent death, Didymus accepted on the authority of Zopyrus and probably of others; his own opinion, that Thucydides returned from exile and died at Athens, which was contrary to what Zopyrus said, he tries to support by argument. Just so in section 3 Pherecydes and Hellanicus are cited apparently in support of the statement that Thucydides traced his descent from the gods. But the words actually quoted concern only the connection between Miltiades and Philaeus, the son of Ajax, who, as everyone knew, was grandson of Aeacus the son of Zeus. As for the connection between Thucydides and Miltiades, Marcellinus or Didymus tries to prove this by inference from a passage in Herodotus, and later from the grave in Koile. If anyone thinks that this is not the most obvious meaning of the first sentence in section 32 ("But Didymus says, etc."), it may be observed with Gilbert that Marcellinus seems to have made a rather careless abridgment of a long argument in Didymus which, in its original form, may have been clear enough.

The idea that the grave in Koile, believed by some to be that of Thucydides the historian, was a cenotaph is evidently the result of an attempt to reconcile two conflicting opinions, namely, that Thucydides died in Thrace and that he was buried among the graves of Cimon's family at Athens.<sup>2</sup> Didymus properly rejected this compromise. Believing that the historian was really buried in Koile, he rejected the

<sup>1</sup> *Op. cit.*, p. 264. Gilbert properly observes that the words *τοῦτο δὲ Ζώπυρον ἱστορεῖ* may have been originally a marginal gloss, by some unknown person. For other explanations see L. Herbst, *Philologus*, XLIX (1890), 375 (cf. Stahl, *ibid.*, L, 36); Fr. Susemihl, *ibid.*, LIX (1900), 538; and W. Schmid, *ibid.*, LX (1901), 155.

<sup>2</sup> U. von Wilamowitz, *Hermes*, XII (1877), 351 f.; O. Gilbert, *op. cit.*, pp. 261 and 264; F. Susemihl, *Philol.*, LIX (1900), 539; W. Schmid, *ibid.*, LX (1901), 157.

testimony of Zopyrus (and Cratippus) that Thucydides died in Thrace. But Didymus was mistaken; the grave in Koile, on the evidence of which he relied, did not really exist. As Wilamowitz pointed out,<sup>1</sup> if there had been a grave at Athens, with an inscription upon it showing it to be the grave of Thucydides the historian, Hermippus, who wrote after 205 B.C.,<sup>2</sup> could not have said that this Thucydides belonged to the family of Peisistratus, and Timaeus, who lived at Athens for fifty years from about 312 B.C.,<sup>3</sup> could not have said that Thucydides died in Italy. Unger<sup>4</sup> and others have thought that the statement of Timaeus refers to some other Thucydides. If the statement be considered by itself, that might well be thought the case. But whoever first quoted the passage from Timaeus must have known to whom it referred; this person apparently believed that it referred to Thucydides the historian, for otherwise it would not be quoted by Marcellinus in connection with the historian, and there is no evidence to the contrary. Doubtless Hermippus' statement is merely a deduction from the tolerant view taken by Thucydides of the rule of the Peisistratidae, and his criticism of Harmodius and Aristogeiton in vi. 54-59. And doubtless the statement of Timaeus that Thucydides was buried in Italy is mistaken. Nevertheless, neither statement could have been made if the grave with its inscription had existed at the time. The belief that there was such a grave must have arisen not earlier than the third century before Christ and perhaps much later, through some misunderstanding which we cannot now discover. How much mischief has been caused by such misunderstandings and by illogical deductions from them is illustrated by Marcellinus (sec. 17): "By the so-called Melite gate there are in Koile the so-called Cimonian monuments, where is shown the tomb of Herodotus and Thucydides. Evidently he [Thucydides] is found to be really of the family of Miltiades, for no alien is buried there." The same argument would, of course, show that Herodotus, a native of Halicarnassus, was of the family of Miltiades.

If the grave of the historian in Koile was imaginary, perhaps the epitaph was imaginary too: Θουκυδίδης Ὀλόρου Ἀλιμουσίου (ἐνθάδε

<sup>1</sup> *Op. cit.*, p. 339. Gilbert, *op. cit.*, p. 243, says that much the same conclusions were reached by Eugen Petersen in his *De vita Thucydidis* (Dorpat Program, 1873).

<sup>2</sup> Christ, *Geschichte der Griech. Lit.* (6th ed.), II, 84, n. 3.

<sup>3</sup> *Ibid.*, p. 219.

<sup>4</sup> *Op. cit.*, p. 104.

κεῖται). But it was not wholly an invention. Plutarch, *Cimon*, chapter iv, in telling about the grave, adds: "However, Thucydides belonged to the deme of Halimus, the family of Miltiades to that of Laciadae." An inscription composed for an imaginary grave in the cemetery of the Philaetae would not have stated that the person buried here belonged to a deme different from that of the others whose graves were in this place. Thucydides does not mention his deme in his own book. His deme, therefore, must have been known in some other way, and it is possible that this inscription really was on the grave of Thucydides, wherever his real grave was.

But if the belief that the historian Thucydides was buried in the grave in Koile originated in the third century before Christ or later, then the opinion that he died in Thrace must have been earlier. For no one who believed that the historian was buried in Koile would have invented the idea that he died in Thrace, whereas anyone who accepted the evidence of the grave and the epitaph as conclusive might well have rejected an earlier report that Thucydides died abroad. The person responsible for this earlier report seems to have been Zopyrus. No one was more likely to know where and how the historian died than a contemporary of his. It seems most probable, therefore, that Zopyrus was a contemporary of Thucydides, although his account was either unknown to or rejected by Timaeus a century later. From the words of Marcellinus (or of Didymus), *κἂν ἀληθεύειν νομίζει αὐτόν* (sc. *Ζώπυρον*), most scholars have drawn the conclusion that Cratippus was later than Zopyrus, for otherwise he could not have known whether Zopyrus spoke the truth or not.<sup>1</sup> Personally I doubt whether more is meant by *ἀληθεύειν* than that Cratippus and Zopyrus agreed. In any case there is no reason to think that Zopyrus could not have published his work before Cratippus wrote, even if both were

<sup>1</sup> J. M. Stahl, *De Cratippo historico*, *Progr.* (Münster, 1887-88), p. 5; *Philologus*, L (1891), 34 ff.; L. Herbst, *op. cit.*, p. 373; Fr. Susemihl, *Philologus*, LIX (1900), 541; H. Weil, *REG*, XIII (1900), 1-9; Grenfell and Hunt, *Oxyr. Pap.*, V (1908), 140; L. F. Benedetto, *Atti d. Accad. di Torino*, XLIV (1908-9), 392; Eduard Meyer, *Theopompos Hellenika* (1909), p. 128; E. Schwartz, *Hermes*, XLIV (1909), 499 ff. Benedetto, E. Meyer, and others held that Cratippus lived in the Hellenistic period. Stahl thought that he lived in the time of Didymus (see below). Schwartz considered him a late forger. On the other hand, G. F. Unger, *op. cit.*, pp. 105-7, believed that Marcellinus was mistaken, and the Zopyrus was later than Cratippus; Marcellinus found in his source that the two agreed, and falsely assumed that Cratippus was the later. So also W. Schmid, *Philologus*, XLIX (1890), 21 n.; LII, 123; LX, 155; V. Costanzi, *Studi Storici per l'Antich. Class.*, I (1908), 261; J. H. Lipsius, *Berichte d. Verh. d. Sächs. Ges. d. Wis.* (1915), p. 24; also C. Müller in *PHG*, II, 75-78.



contemporaries of Thucydides. We know absolutely nothing about this Zopyrus except from these references in the Marcellinus biography.<sup>1</sup> From the form of the condition, *κἂν ἀληθεύειν νομίζει*, Stahl, believing that these were the words of Didymus, argued that Cratippus had not yet expressed agreement with Zopyrus but might still do so, and that therefore Cratippus was still living when Didymus wrote.<sup>2</sup> Herbst, who believed that these were the words of Marcellinus himself, thought it possible that *κἂν νομίζει* is used as equivalent to *κεῖ νομίζει*.<sup>3</sup> Unger believed that these words indicate that Marcellinus had not read Cratippus himself, but only learned from others what he said.<sup>4</sup> My own opinion is that the form of the condition in *κἂν νομίζει* implies that the author, whoever he was, was not entirely sure what Cratippus meant. Like some modern scholars he may have tried to explain the statement of a more ancient writer in accordance with a theory of his own, and may have had a suspicion that not everyone would agree with him, because there was testimony to the contrary which was hard to ignore.

One other fact in this connection remains to be noticed, namely, that although in section 33 Marcellinus either gives as his own opinion, or quotes without dissent an opinion of Didymus, that Zopyrus was talking nonsense when he said that Thucydides died in Thrace, yet in section 45 the statement is made without explanation that Thucydides did die in Thrace. That only proves that Part II of the Marcellinus biography, which begins with section 45, is not by Marcellinus but by someone else, and that is clear enough anyway.

It seems to me, then, that there is no evidence at all in Marcellinus (secs. 30-33) that either Zopyrus or Cratippus wrote later than the early part of the fourth century before Christ. On the contrary, the natural inference from this passage is that both were contemporaries of Thucydides, as Unger and Herbst believed.

Dionysius of Halicarnassus, *De Thucydidis characterē, etc.*, chapter xvi, says that the work of Thucydides is not of uniform quality: certain parts of his work are above reproach, while other parts show an inferior talent, especially in the speeches, the dialogues, and other rhetorical passages. He then continues as follows:

<sup>1</sup> Herbst, *op. cit.*, p. 373.

<sup>2</sup> *Progr.*, pp. 6 f.; *Philologus*, L, 42.

<sup>3</sup> *Op. cit.*, pp. 344 f. But see Stahl, p. 42, and Schmid, *Philologus*, LII, 121-23.

<sup>4</sup> *Op. cit.*, p. 106.

ὡν προνοούμενος ἔοικεν ἀτελῇ τὴν ἱστορίαν καταλιπεῖν, ὥς καὶ Κράτιππος, ὁ συνακμάσας αὐτῷ, καὶ τὰ παραλειφθέντα ὑπ' αὐτοῦ συναγαγὼν, γέγραφε, οὐ μόνον ταῖς πράξεσιν αὐταῖς ἐμποδῶν γεγενῆσθαι λέγων, ἀλλὰ καὶ τοῖς ἀκούουσιν ὀχληρὰς εἶναι. τοῦτό γέ τοι συνέντα αὐτόν, ἐν τοῖς τελευταίοις τῆς ἱστορίας φησὶ μηδεμίαν τάξαι ῥητορείαν, πολλῶν μὲν κατὰ τὴν Ἰωνίαν γενομένων, πολλῶν δ' ἐν ταῖς Ἀθήναις, ὅσα διὰ λόγων καὶ δημαγοριῶν ἐπράχθη.

Apparently it was because he gave attention to these things that he left his history unfinished, as Cratippus also, who was a contemporary of his and brought together what he [Thucydides] left aside, has written, saying that they [the speeches, etc.] were not only a hindrance to the facts themselves, but also troublesome to the audience. And because he recognized this himself, in the last part of his history [Thucydides] included no rhetorical passage, he says, though there was much in Ionia and much at Athens which was accomplished by conferences and public addresses.

I suppose that Dionysius meant that probably Thucydides left his work unfinished because he spent so much time and attention on the speeches and other rhetorical portions of his book. That may be true, and Dionysius may well have thought so, even if he knew, as I suppose he did know, that Thucydides died before his work was completed. The difficulty in the passage arises with the words: "As Cratippus also . . . has written, saying. . . ." For the words quoted from Cratippus do not say at all what Dionysius has just said. It seems to most of us entirely obvious what Cratippus meant. The speeches do get in the way of the narrative, delay the presentation of subsequent events until sometimes the connection with the events preceding is obscured, and the interest of the hearer or reader is lost. Moreover, the speeches are troublesome to the audience—we can all testify to that. They not only increase the difficulty of following the sequence of events, but they are often very hard to understand; most readers feel a distinct relief when, certain speeches past, the historian returns again to the lucid narrative. Cratippus, so far as he is quoted here, says nothing at all about the incompleteness of Thucydides' book, or the reason for its abrupt ending in the midst of the events of 411 B.C. How, then, could Dionysius assert that Cratippus agreed with him in saying that Thucydides left his work unfinished because he gave so much time and thought to the speeches? I think that Benedetto has found the right answer to this question.<sup>1</sup> Dionysius

<sup>1</sup> L. Foscolo Benedetto, "Lo Storico Cratippo," *Atti d. Accad. di Torino*, XLIV (1908-9), 378.

foolishly thought that, in saying that the speeches were a hindrance to the facts, Cratippus meant that the speeches prevented the narrative from reaching its natural conclusion at the end of the war in 404 B.C. On the other hand, there seems to me to be no justification for Benedetto's conclusions that Cratippus failed to appreciate the purpose of the speeches in Thucydides and therefore belonged to the Alexandrine age.<sup>1</sup> As stated above, Cratippus said only that the speeches impeded the events, that they were difficult for the hearers, and finally that Thucydides omitted the rhetoric from the last part of his work because he was conscious of these defects. The last opinion is probably incorrect; but no one can say with certainty that Thucydides did not intend to omit speeches in direct discourse from the eighth book, even if he believes that death prevented him from completing this book as he wished. The other statements of Cratippus are true, and might have been made at any time. Even Plato held this opinion. Such criticisms of rhetorical composition were certainly not more natural in the Alexandrine or the Roman periods than in the early fourth century before Christ.<sup>2</sup> There is, therefore, no evidence in the passage from Dionysius against the unqualified statement of that author that Cratippus was a contemporary of Thucydides.

Those who would assign Cratippus to a later date find it somewhat difficult to explain, in accordance with that opinion, a passage in Plutarch, *De gloria Atheniensium*, chapter 1, where Cratippus is mentioned between Thucydides and Xenophon in a list of Athenian historians which appears to be arranged in chronological order. Mr. W. J. Oates, a graduate student working with me, remarked that the rather complicated arguments of these scholars<sup>3</sup> are justified only if there is some evidence elsewhere against the natural implications of this passage. There is no convincing evidence of this sort. The composition of the passage in question is perfectly natural and comprehensible as it stands, if Cratippus was in fact an Athenian historian who wrote a little later than Thucydides and a little earlier than Xenophon.<sup>4</sup>

<sup>1</sup> *Op. cit.*, pp. 379, 385, and 395. With Benedetto agree in the main C. Müller, *PHG*, II, 75 ff.; R. Schöll, *Hermes*, XIII (1878), 446; Stahl, *Progr.*, p. 4, and *Philologus*, L, 30; E. Meyer, *op. cit.*, p. 129; and others.

<sup>2</sup> Unger, *op. cit.*, p. 106; Schmid, *Philologus*, LII, 120, and LX, 156; J. H. Lipsius, *Verh. d. Sächs. Ges. d. Wis.*, LXVII, 16-22, and *Cratippi Hell. Fragg. Oxyr.* (1916), pp. 2 f.

<sup>3</sup> Best presented by Benedetto, *op. cit.*, pp. 386-89. <sup>4</sup> Cf. Lipsius, *op. cit.*, p. 15.

Lastly, an argument for the later dating of Cratippus has been drawn from a scholium attached to the *Life of Andocides* by the Pseudo-Plutarch.<sup>1</sup> Benedetto<sup>2</sup> admits that the scholium is corrupt, but thinks we can clearly see that according to Cratippus the Corinthians by night mutilated the "Hermae," and, since there is no trace of such an idea in better historians, concludes that it is false and late. But, as Lipsius<sup>3</sup> observes, this story may have been mentioned, not in an account of the mutilation of the *Hermae*, but in connection with some later event, such as the trial or conviction of Andocides—perhaps it was part of the argument for the defense. In any case the evidence of this scholium is too uncertain to be used in determining the date of Cratippus.

I believe, therefore, that Cratippus, and probably Zopyrus also, were contemporaries of Thucydides. Their testimony, then, that Thucydides died in Thrace, by a violent death, may be added to the little we can learn about the life of the great historian from his own statements about himself in his own book. There is nothing else which can be added, with any degree of certainty, if we except the statement that his deme was Halimus, and whatever anyone can derive from the mutilated passage in Pausanias i. 23. 11.

In conclusion, this discussion of the date of Cratippus has a bearing on the problems which have emerged in the controversy over the authorship of the *Hellenica Oxyrhynchia*. I believe that Eduard Meyer has demonstrated that this papyrus is most probably a part of the lost *Hellenica* of Theopompus. But Meyer has also shown<sup>4</sup> that both Theopompus and Xenophon had for their *Hellenica* a great deal of material no longer known to us, sources of which even the titles and perhaps the authors' names are lost. It is quite possible that among these sources Cratippus' work should be counted.

#### PRINCETON UNIVERSITY

<sup>1</sup> Plutarch, *Moralia*, p. 834 = Bernadakis, V, 151. In a scholium to Aristoph. *Lys.* 1094 the same story seems to be derived from Philochorus.

<sup>2</sup> *Op. cit.*, pp. 392 f.

<sup>3</sup> *Op. cit.*, p. 16.

<sup>4</sup> *Op. cit.*, esp. pp. 49, 145 ff., 154 f.

## NOTES AND DISCUSSIONS

### THE BIRTH CERTIFICATE OF A ROMAN CITIZEN

In the *Transactions of the American Philological Association*, LIV, 187-95, Professor Francis W. Kelsey published a most interesting wax tablet of the year 128 A.D. Not all the difficulties could be solved by him, nor have they been properly treated by others up to the present, the last publication being by Schubart in *Griechische Papyri, Urkunden und Briefe* (Leipzig, 1927), No. 33.

As a basis for my few remarks I reproduce the text of the tablet as first published.

MICH. PAP. 766

(Pag. I)

L Nonio Torquato Asprenate II M Annio  
Libone Co(n)s(ulibus) Idib(us) April(ibus) anno XII Imp(eratoris)  
Caesaris Traiani Hadriani Aug(usti) mense  
Pharmuthi die XVIII Alex(andriae) ad Aeg(yptum)  
descriptum et recognitum ex tabula pro-  
fessionum quibus liberi nati sunt  
quae tabula proposita erat in foro Aug(usti)  
in qua scriptum fuid id quod infrascriptum  
est M Claudio Squilla Gallicano  
T Atilio Rufo Titiano Co(n)s(ulibus) anno Imp(eratoris)  
Caesaris Traiani Praef(ecto) Aeg(ypti) professiones liberorum  
acceptae citra causarum cognitionem

(Pag. II)

tab(ula) VIII pag(ina) II amplioribus litteris  
scriptum est L Nonio Torquato Asprenate  
II M Annio Libone Co(n)s(ulibus) et post alia pag(ina) IX  
VI Kal(endas) April(es)  
C Herennius Geminianus HS ccclxxv  
fil(iam) n(atam) Herenniam Gemellam  
ex Diogenide M(arci) fil(ia) Thermu-  
thario V Idus Mart(ias) q p  
f c a (or r) e ad k

We must first note that the dots marking abbreviations are still visible in most places either on the wax or on the wood. This is important only on the last phrase, which is read plainly *q. p. f. c. r. e. ad k*. The first three letters Hunt (Oxyr. Pap. VIII, No. 1114) has already expanded as *q(uae) p(roximae) f(uerunt)*, and he has been followed by all others, though in some

cases, as here, the addition of other letters makes the interpretation doubtful. The whole seems to belong to the same sentence, if not to the same phrase. So Viereck, now followed by Schubart, has expanded *q(uae) p(roximae) f(uerunt) cre(scentes) ad K(alendas)*. The first three words have a natural meaning, equivalent to the phrase "last past" or "just past" following the day of the month in English documents, but I do not comprehend the meaning or the relevancy of the last three words. Moreover, in our tablet the letters *c, r, e* are separated by dots and so must stand for three separate words.

Professor G. Dittmann, in a long and most carefully prepared letter to Professor Kelsey, has handled the abbreviation *ad k*. He shows that *ad K(alendas)* is most improbable and inclines to *ad k(alendarium)*, though somewhat doubtful about its meaning until the rest of the abbreviation is settled. From the abundant material that he furnishes I quote *CIL*, X, 5657, 3, as proof that the abbreviation can be so expanded.

If we turn now to the text of the document it is plain that the verb is missing in the last sentence, and that there is no mention of the important fact on account of which such birth certificates were secured, namely, to prove Roman citizenship. We are therefore entitled to expand *c. r.* as *c(iuem) R(omanam) or R(omanum)* in all such birth certificates. The letter *e* accordingly stands for the missing verb and may be expanded *e(xscripsi)* or *e(xscripsit)*, meaning that "I [name of the father] have written out for the public monthly record that a son was born, etc." The preposition *ad* would imply a condensation, meaning rather "for publication in."

What, then, was the *Kalendarium*? I believe that this document tells us quite fully. It was the *tabula, quae proposita erat in foro Augusti*, etc. This was exhibited in the Forum of Augustus for the purpose of publication, yet it or a copy of it was preserved there, for as we see later a copy of the record was made with reference to tablet and page.

The record was kept according to the years of the emperor; note that this document was dated in the twelfth year of Hadrian. Trajan had died at the end of July, so the first month of Hadrian's rule was August. March was the eighth month of the emperor's year. Accordingly, this birth on the eleventh of March was quite properly recorded in the eighth *tabula* of the record. The record was a monthly record, one tablet for each month. For this reason it was called a *Kalendarium*.

On page 2, lines 8 and 3, of the wax tablet there are two citations of the record, *tabula viii, pagina ii . . . . . et post alia pagina ix*, the second of which was made on March 27 (*VI Kal April*). If we assume that the other was made soon after the birth, March 11, then seventeen days might be considered the regular duration of the publication after which the final entry could be made in the record, which would guarantee citizenship and property rights. The first entry would be merely the assertion of birth by the father or his representative; the final entry would be more elaborate, containing the full names of father and mother, rating, dates, and other evidence obtainable.



So we understand why the first entry fell on page 2, though the birth came in the eighth month of the year, while the second entry came on the ninth page. The final entry was the official one and occupied about three times as much space as the original entry.

In the ninth and tenth lines of the first page of the wax tablet the consuls for the year 127 A.D. are given, though the birth and the whole record came in the year 128. Both consular years are stated as the twelfth year of the emperor. The explanation of this is now obvious, as Schubart appears already to have seen in the work cited above. The whole title of the record-book was given at the head of the first tablet of the year (August, 127 A.D.) as we now read it in lines 5-13 of the first page of the wax tablet. As the twelve tablets for the year formed the *Kalendarium* and were kept as one, this title would not need to be repeated on the succeeding tablets, or in the book or roll, if the tablets were copied for preservation and easy of reference. Naturally it was not necessary to copy the names of the consuls of 127 after the beginning of 128, but as they formed a part of the title of the volume it would be entirely natural to copy them. Their presence here helps to confirm the picture of the official record, in which the births of Roman citizens were set down after due publication, and which we have seen above was probably called a *Kalendarium*.

These results are supported by the contents of three birth certificates on wax tablets published by Viereck and Zucker in Volume VII of the *Ägyptische Urkunden*. The general type and formulas of all these tablets are most similar to the Michigan tablet just treated, but they are not as well preserved nor do they seem to have been as carefully written.

Number 1691 records the birth of a son on June 9, 109 A.D., the twelfth year of Trajan. According to the editors, the record was made *tabu(la) I. item pagina II / A. Cornelio Palma Frontoniano [II P. Calvisio Tullo] / Rusone co(n)s(ulibus). V K(alendas) / Julias*. This should be read *tab(ula) V (pagina) I* or possibly *tab(ula) VI*. Nerva died on January 27, so the first month of Trajan's rule was February, and June, the month both of birth and registration, was the fifth. Also a page number seems desirable here because of the following notice introduced by *item*, which was made on June 27, and further there must have been space and perhaps a dot after *u* in *tabu(la) I* or the editors would have expanded the abbreviation in the regular manner, *tab(ula) VI*. Again the double entry in the record indicates a preliminary publication of the father's statement and later official registration after the legal interval. If the legal interval was seventeen days, the preliminary publication came in this case two days after the birth. The page number of the final registration ought to be higher on the analogy of the other tablets. At the end of this tablet *q(uae) p(ro)zuma(e) f(uer)unt* is followed by the name of the prefect of Egypt, partly in lacuna. The customary abbreviation *c. r. e. ad K.* probably followed in order to complete the formula.

Number 1692 records the birth of a son on August 18, 144 A.D., in the



eighth year of Antoninus Pius. In lines 13 f. of the tablet stands: *tab(ula) II et post alia pag(ina) II item anno VIII Imp(eratoris) Antonini domi/n(n)ostri isdem co(n)s(ulibus) item pag(ina) III Idibus Septembr.* As given the registration came on September 13, the third month of the imperial year of Antoninus Pius, but it appears on *tab(ula) II*. However, there seem to have been three entries here in the *Kalendarium*, and the first should have been at least the legal publication period before the last, which is the one dated September 13. A publication period of seventeen days would put the first notice on August 28—ten days after the birth. The first notice may have contained an error and hence the need of the second notice, which would have fallen on or about August 28. Both would come in the same month as the birth and naturally be registered on *tab. II*. The failure to record the page number of the first entry is unimportant as it must have been page I and so could have been inferred. At the end of line 9 it is necessary to correct the reading of the tablet to *anno VI [II]* as all entries as well as the title of the entry-book must fall in the same year of the Emperor. In the last line of the tablet the abbreviation should be expanded *q(uae) p(roxumae) f(uerunt) c(ivem) R(omanum) e(xscripsi) ad K(alendarium)*, as noted in the discussion of the Michigan tablet.

Number 1694 records the birth of a son on July 19, 163 A.D., in the third year of Marcus Aurelius (*anno IIII* of l. 2 must be an error; *ann[o] III* of l. 8 is right). In line 13 the editors read *tabul(a) IIII pag(ina) V V Id(us) Aug.* If this is correct, the birth was put on the tablet for July, the fourth month of the imperial year of Marcus Aurelius, though it seems not to have been recorded until August 9 in the fifth month. I prefer to assume an abbreviated copy here, which omitted the first notice of registration but gave the page and date of the final record, some seventeen days later. The first notice would have fallen on or before July 24, five days after the birth, and so naturally have come on *tab. IIII*. The end of this tablet contains an additional item, the home: *domicil(io) Jul(iopoli) Aeg(ypti)*, which is placed between *q(uae) p(roxumae) f(uerunt)* and *cre(scentes) ad K(alendas)*. This should have warned the editors against their expansion of the abbreviation. On the other hand, the phrase *c(ivem) R(omanum) e(xscripsi) ad K(alendarium)* follows quite as naturally after the mention of the residence. In all of these tablets the father's name in the nominative case calls for a verb before *ad K(alendarium)*, and the declaration would naturally be made in the first person. The verb *professus est* is used in describing the act of the father but need not be the one he would have used in the formal declaration.

Number 1693 is only half a tablet and does not contain the reference to *tabula* and page in the record-book nor the following portion of the certificate. The copy was made on May 17, 145 A.D. The mention of the consuls for 144 A.D. in line 8 is to be explained as a part of the title of the record, as in the Michigan tablet discussed above. The child was probably born in April or possibly in March and not necessarily in the previous year as the editors state.

The registration of birth and securing of certificate seem to have occurred fairly promptly in the case of all the birth certificates preserved except No. 1694, where the father was absent at the time of the birth.

The one remaining tablet seems opposed to my interpretation. Cairo 29807 was first published by Seymour de Ricci in *Nouv. revue hist. de droit*, XXX (1906), 483 ff. Professor Wilchen (*Archiv*, IV, 251) criticized and corrected it. The most recent publication is by Meyer, *Juristische Papyri*, No. 4. In his original publication De Ricci states that the tablet was much mutilated and difficult to read and that his transcript had to be made hurriedly. The tablet does not seem to have been compared by any scholar since. It records the birth of a daughter on August 20, 148 A.D., and the registration, *tabula V et post alia pag(ina) III*, on September 14. The year is further designated as the twelfth of Antoninus Pius. According to the system assumed for the tablets just discussed, the record of birth should have been on *tabula II* or *III*. August, the second month of the imperial year of Antoninus Pius, was the month of the birth and presumably of the first record, which would be on the first page of the tablet, though here not mentioned. The second and final record (*post alia*) came on page III. Two entries on different pages are definitely implied, and they would seem to have been separated as usual by a reasonable period for legal publication, as seventeen days. For that reason I prefer to emend to *tabula II* rather than to *tabula III*. Also the confusion of  $\overline{\text{II}}$  (= V) as it is written in large letters on wood, with  $\overline{\text{II}}$  (= II) is much easier than with  $\overline{\text{III}}$ . Where not effaced the writing on the wood was doubtless easier to read than that on the wax. At the end of the tablet there is the tentative reading *jr ad F*. In all probability this should be read *[c] r e ad K*, as in the other birth certificates. It is immediately preceded by *q(uae) p(rozumae) f(uerunt)*. According to my interpretation, this formula is a necessary termination of every birth certificate.

UNIVERSITY OF MICHIGAN

HENRY A. SANDERS

#### ON SOME FRAGMENTS OF CATULLUS

There are three fragments in Catullus, 2b, 14b, and 78b, which have elicited much discussion. The question regarding 2b is whether or not it can be a part of 2. In the case of 14b the unpalatable suggestion has been made that it might be attached to 2b. The usual solution is to consider it a fragment of a dedication, in which case it is assumed that Catullus issued at least two *libelli*. The other fragment, 78b, first separated by Statius, has had to travel about a great deal, but seems not to find a resting-place.

I believe that there is one rather definite fact about the arrangement of Catullus' poems which will aid us in making a satisfactory decision about all three of these fragments. When two poems on similar themes occur in Catullus the editor, whoever he was—I do not think he was Catullus—generally

separated them by one poem on an alien theme. This rule, which seems to have no exceptions in the first half of the volume, holds good for 5-7, 16-21 (18-20 are not the work of Catullus), 21-23, 24-26 (24. 5 refers to Furius), 37-39, 41-43, 69-71, 70-72, 71-73, 107-9, and possibly for other couplets. We should therefore expect the *passer* poems, 2 and 3, to be separated in the same manner, and I think they were. In 2*b* we evidently have the end of that separating poem composed on a different theme.

In 14*b*:

Si qui forte mearum ineptiarum  
Lectores eritis manusque vestras  
Non horrebitis admovere nobis,

we have of course an apologetic address to his readers that itself is very unusual in Catullus. But in 16 we have the well-known apology for the immodesty of his verses to Juventius. The substance is, to use Herrick's imitation: "Jocund his Muse was, but his life was chaste." I think we may take No. 15 as a separating poem and consider 14*b* not as a dedication of a second volume but rather as a part of an explanatory poem to correspond to No. 16. Here of course the logic is less compelling than in the preceding case, for I have no intention of "converting A simply," as Jevons would call it. All we can say is that 14*b* and 16 look like one of the numerous pairs.

I should apply the same principle to 78*b* with the same caution. No. 77 is an epigram upon Caelius Rufus who has now succeeded Catullus in the affections of Lesbia. The fragment in question is separated from that one by an epigram on an alien subject and is most easily explained as an attack upon Caelius Rufus. Doubtless two or four lines have been lost but its position seems to me a fairly good indication of what its theme probably was, and I should not hesitate to add it to the Caelian group.

JOHNS HOPKINS UNIVERSITY

TENNEY FRANK

#### SOME REMARKS ON A PASSAGE IN PARTHENIUS

The first sentence in Parthenius 29 reads as follows in the text of Martini, which agrees with the MS: *ἐν Σικελίᾳ δὲ Δάφνις Ἑρμοῦ παῖς ἐγένετο, σὺριγγι δὴ τε δεξιῶς χρῆσασθαι καὶ τὴν ἰδίαν ἐκπρεπῆς*.

A number of attempts to better the obviously corrupt *δὴ τε* are recorded in Martini's note: *τε δὴ* (Passow), *δὴ τε* (Jacobs), *δὴτα* (Martini), *δὴ [τε]* (Soaliger), *[δὴ] τε* (Legrand); some adjective like *ἐπιτήδειος* (Kayser). Instead of discussing these various suggestions, let me venture simply to set beside them one which I hope will command assent at once. What Parthenius wrote was surely *σὺριγγι (ψ) δὴ τε*. By this reading we get in exchange for an otiose *δὴ* something the very reverse of otiose. The bucolic muse was wooed

not only with the pipes but with song, of course; and Daphnis was adept at both. Does not Theocritus say of Daphnis and Menalcas: ἄμφω συρίσδεν δεδαημένω, ἄμφω δαΐδεν? In Diodorus (iv. 84. 4) we find both mentioned: μυθολογοῦσι δὲ τὸν Δάφνιν μετὰ τῆς Ἀρτέμιδος κυνηγεῖν, ὑπηρετοῦντα τῇ θεῷ κεχαρισμένως, καὶ διὰ τῆς σύριγγος καὶ βουκολικῆς μελωδίας τέρπειν αὐτὴν διαφερόντως. Both are to be found also in the comment of Philargyrius on Vergil *Eclogue* v. 20 (Servius [ed. Lion] ii. 326): "quod [his blindness] licet carminibus et fistula solaretur, non tamen diu vixit." This consensus makes it sufficiently evident, I trust, that in Parthenius σύριγγι by itself does not do full justice to Daphnis, but requires to be supplemented by precisely this word ὠδή which is so easily obtained from the bothersome δῆ.

One further change is necessary: Provision must be made in some way for the government of χρήσασθαι, which cannot be taken to depend on ἐκπρεπής. The best way, I think, is to adopt Gale's alteration of δεξιῶς into δεξιός. The construction, though unusual, is perhaps, for Parthenius, sufficiently guaranteed by Procopius, who thus describes the dexterity of Peter, called Barsymes, in the matter of giving short change: δεξιός γὰρ ἦν κλέψαι μὲν τὰ [the coppers] τῶν αὐτῷ περιπεπωκότων ἀναΐδην, ἀλοῦς δὲ ὁμόσαι καὶ τῶν χειρῶν τὸ ἀμάρτημα τῷ τῆς γλώττης περικαλύψαι θράσει.<sup>1</sup>

The text, then, will read as follows: σύριγγι ὦδῃ τε δεξιός χρήσασθαι καὶ τῇν ιδεάν ἐκπρεπής.

In the use of τε to connect σύριγγι and ὦδῃ just as -que is used to connect two words in Latin there is nothing exceptionable. It is so used in the prose of Plato as well as in poetry. In the *Phaedrus* (267A) there is a very pretty example: Τεισίαν δὲ Γοργίαν τε ἐάσομεν εὔδειν. Parthenius, himself a poet and given to poetical turns of expression, is especially fond of this idiom: ἀνδρὸς ἐν Μιλήτῳ πάντῳ δοκίμου γένους τε τοῦ πρώτου (viii. 2) (ὑπέσχετο) μηδαμὰ προλείψειν ἐν περισσοτέρᾳ τε τιμῇ ἄξειν (iv. 2) κατακρύψαι τὰς βοῦς μὴ θέλειν τε ἀποδοῦναι (xxx. 1).

Exception cannot be taken, either, to the use of δεξιός in connection with song, which is to be explained not as zeugma but as catachresis, and is supported with entire adequacy by Lucian *adv. Indoct.* 10: ἀλλ' οὐτός γε ἤσας δεξιῶς καὶ κιθαρίσας κατὰ τὸν νόμον τῆς τέχνης ἐκράτει, κτλ.

In other respects I see no reason to question the text. For χρήσασθαι Scaliger wrote χρῆσθαι, but the aorist seems no more open to criticism than in Homer when he says: οἷδ' ἐπὶ δεξιᾷ, οἷδ' ἐπ' ἀριστερὰ νομῆσαι βῶν (*Iliad* vii. 238). And we must certainly decline to rob the style of Parthenius and the beauty of Daphnis of distinction by changing ἐκπρεπής to εὐπρεπής. That alteration is one which frequently seemed to scribes a happy idea, and is best left to them; see Euripides, *Alcestis* 333 and *Hecuba* 269, with the variant readings.

A. M. HARMON

YALE UNIVERSITY

<sup>1</sup> *Hist. Arcan.* 22.

### A HYPOTHETICAL CHRONOLOGY FOR THE YEAR OF THE GORDIANS

The exact dates of the events of the troubled year 238 A.D. have been stated differently by practically every author that has undertaken to deal with the period; the present writer, therefore, submits without apologies what seems to him the most probable chronological scheme for this eventful year, even at the risk of adding another to the already confusingly large number of such hypotheses worked out by others.

The literary sources do little to help us in this regard. Herodian tells a clear enough tale, with few or no dates; the *Scriptores Historiae Augustae* as usual twist and bungle, though often using good sources; the Epitomist of Aurelius Victor knows the story correctly, but without accurate chronology; the rest do but display their ignorance of the most elementary facts in the case. Coins as usual can be used only as corroborative evidence or to date approximately; while not one dated inscription has been found entire to settle definitely the disputes that have arisen. Any solution, therefore, that is offered must be considered as more or less hypothetical, and valuable only in so far as it is not contradicted by positive evidence and is supported by whatever evidence there is.

The *Historia Augusta* (Max. et Balb. 1. 1) states the date of the election of Maximus and Balbinus as July 9, and states further that Gordian III was made Caesar the same day. The same authority states (*Gordiani Tres* 23. 2) that the day Gordian III was made Caesar an eclipse of the sun occurred. Now these statements are mutually incompatible. Astronomical records show that no eclipse occurred on July 9, 238; but that one did occur on April 2, 238; while the Alexandrian coins show that the ninety-nine days allowed to Maximus and Balbinus by the Chronographer of 354 must have expired before August 29, 238, since only coins of the first year of their reign have been found. Add to this the fact that seven legal signatures of Gordian III are found in the Justinian Code with dates varying from March 29 to August 8 (vii. 43. 2; ii. 10. 12; ix. 1. 8; v. 51. 5; vi. 3. 11; ii. 22. 2) and we see that the election could not have taken place on the date above mentioned, but must have been much earlier.

A curious coincidence at once becomes apparent, however. From the actual date of the eclipse till the day when it is said to have occurred is exactly, by the Roman method of computation, ninety-nine days—the length of the reign of Maximus and Balbinus as stated by the Chronographer of 354. This furnishes a starting-point for further investigation. Let us assume that the slovenly hack who compiled the life of Maximus and Balbinus mistook in the chronicle from which he got the materials for his work the date of the deaths of these two emperors for that of their election. Such a performance would not be without parallels elsewhere in that collection. Will the assumption that

they were elected on April 2 and killed on July 9 meet all the conditions laid down in the available data?

It is in agreement with the coins, which were all struck within the course of one Egyptian year (August 29–August 28). It will meet all conditions imposed by the dates of the legal signatures—remembering always that these show Gordian III to have been sole emperor at the dates of their making—except the first two. Now mistaken dates in the signatures of the Justinian Code are not uncommon. Thus iii. 42. 6 is a rescript of Philip, dated March 13, 244; while vi. 20. 6 is signed by Gordian III, Philip's predecessor, on May 7 of the same year! We must therefore lay down some rule to govern such cases; and a fair assumption would be that though one signature, unsupported, is insufficient to establish the fact that an Emperor is on the throne, a large number, fairly close together, ought to furnish strong presumptive evidence to that effect. Now the first signature is dated March 29, and there are no more till June 22; then no more till July 16. But thereafter they come on July 23, August 3, August 6, and August 8. Our first two, then, might easily be misdated. The later ones could not easily be so. *CIL* vi. 2113, moreover, records the decennial vows (made regularly at the accession of an emperor) pronounced for Gordian III, on the sixth day before the Ides of, to us, an unknown month, by the Arval Brothers. The promptness of this servile organization in adulation of emperors is so well known that we need hardly wonder if they held a meeting for this purpose the day after their emperor's accession. Hence another piece of evidence fits into the scheme. *CIL* iii. 4820, a fragment from the Danubian Provinces, seems to contradict the foregoing date, being dated June 23, 238, and containing the words "Aug. N."—a reference, it is claimed, to the existence of but one Augustus on that day. The fragment is badly shattered, and the reading a conjectural one. Granted that the piece which we have was correctly read, we cannot make any sense from it, or base any conclusions upon it alone. The bulk of the evidence seems, therefore, to uphold our hypothesis.

If, then, the election of Maximus and Balbinus took place on April 2, we may presume the fall of the Gordians to have occurred about March 28, and their elevation (the Chronographer of 354 says they reigned twenty days), about March 8. In a previous number of this periodical the writer gave his reasons for believing that Alexander Severus was killed on January 8, 235. The Chronographer of 354 says that Maximinus reigned three years, four months, and two days. This would make his death occur about May 10, 235. Again it must be said that this is but a hypothesis; but it seems to satisfy the conditions imposed upon us by the data at hand; and at worst it can but add to the dozen or more such schemes already submitted by others.

C. E. VAN SICKLE

UNIVERSITY OF CHICAGO



## THE INSCRIPTION OF DVENOS

In *Language*, Vol. II, No. 4 (December, 1926), there appeared an article by Professor Roland G. Kent, of the University of Pennsylvania, entitled "The Inscription of Dvenos." The purpose of the article was to interpret this archaic Latin inscription, which has puzzled scholars since its discovery in 1880. To criticize Professor Kent's treatment of the linguistic problems involved lies beyond my competence; I will venture to say, however, that the treatment is obviously brilliant. The purpose of this note is only to suggest what seems to me a more plausible solution than his of an archaeological problem presented by the inscription itself and by the vase on which it is inscribed. To exhibit this problem I will quote the article above cited, abridging slightly for simplicity.

The jar or vase in question is compounded of three jars, forming a small triangle about six inches on the side. The cavities . . . are independent, not communicating with one another. The inscription runs around the outside of the whole vase, in three lines. . . . When the vase is standing on its base, the letters are upside down. [When, however, the vase is inverted, and the letters therefore brought right side up,] the first line stands below the second, and the second below the third. The order of the lines is not in doubt, as the end of the second line is bent out of direction to avoid collision with the beginning of the first line, and the end of the third line is bent away to avoid the beginning of the second line.

In short, when the vase is right side up, the lines also are right side up, but the letters are upside down. How is this peculiarity to be accounted for?

To find an answer to this question, we naturally inquire into the meaning of the inscription. The upshot of the numerous researches on this subject is lucidly summarized, in the article I have quoted, by Professor Kent, who gives as his own interpretation the following:

He who sends me, [implores] the gods Jove, Vejove, (and) Saturn, may the Maid not be kind to thee; but [may she stand aloof] from thee, unless thou wishest to make thy peace with Ops Tuteria or by the help (with the intercession) of the Salutary (Maid). A good man, [Bennus,] made me as a present to the Gods of Hell; to the good man may not evil come to pass because of me.

According to this interpretation, then, the inscription, by a familiar convention, is represented as the utterance of the vase itself; and it is an imprecation on some persons unnamed. As Professor Kent explains, the gods Vejove and Saturn were "associated with the netherworld," and "the Maid" was presumably "Proserpina, as goddess of Hades." The author of the curse (Dvenos, interpreted to mean either "Bennus" or "a good man") is conjectured to have been possibly "a priest execrating a person who has in some way offended the goddess served by the priest." The inversion of the letters seems appropriate to baleful magic; and doubly so, taken in connection with the additional fact that they must be read backward—i.e., from right to left.



Not content, however, with this explanation of the inversion, Professor Kent draws the further inference that

the vase was to be buried with the mouths downward, addressed to the three deities, one of whom was to have a dwelling in each cavity, while operating the appeal on the vase, which in this position was legible to them. It is true that the first line is now physically the lowest, and the third line is physically the highest; but the chthonic deities, rising to inspect the gift, would perceive and read the lowest line first.

A difficulty with this hypothesis is that, on Professor Kent's own showing, the inscription is not "addressed to the three deities." It is addressed to a totally unidentified "thou," while the deities are invoked in the third person. In order, therefore, to "operate the appeal," these deities must be supposed able—without a clue, so far as the inscription itself goes—to divine who was in the mind of the maker or burier of the vase. And this, although they are supposed so limited, in both location and intelligence, as to be unable to read an inscription upside down.

Another difficulty is that if buried with the mouths downward the vase must presumably have been buried empty. Now an empty vase seems a singularly unpropitiatory offering. The deities, "rising to inspect the gift," might not unnaturally have been disappointed!

Both these difficulties can be avoided if we assume that the vase, suitably filled, was buried right side up. Professor Kent, as we have seen, suggests it as possible that the responsibility for its making, inscribing, and burying rested with a priest. Accepting this suggestion, I hazard the conjecture that he was a priest of Proserpina, and that the place of the burial was that precinct of hers which was under his care. There the vase, with its sinister message, would remain innocuous unless disturbed by someone who with spade or plow profaned the sacred inclosure. To this intruder, stooping to remove the obstacle against which his tool had struck, the inscription would then appear. Looking straight downward, he would have no trouble in reading it upside down; indeed, it would be only because it was upside down that, so looking, he could read it. Any reader of this note can demonstrate this fact to his own satisfaction with any book or magazine.

About the application of the menacing words to himself our imagined intruder would have no doubt, for it was not left to the modern adviser to tell the reader of an inscription in which the second person appears that "this means you." If he cleared away the earth only enough to read the first line, he would see the dread names invoked against him, and the execration "may the Maid not be kind to thee." With this much warning, if a prudent man, he would at once cover the vase again, discontinue his sacrilegious meddling with the field it was designed to protect, and offer amends to the tutelary deity. If so venturesome as to persist until he uncovered the second line, he would find the threat repeated, with a definite suggestion that he had better

"make his peace." (The mysterious title "Toitesia" = "Tuteria" may perhaps be rendered "the Protectress"—i.e., Proserpina, guardian of the sanctuary.) The possibility that the curse might recoil on the maker of the vase, perhaps in the very act of burying it, is guarded against by the final words of the inscription—"to Dvenos may not evil come to pass because of me."

The rumor that such a curse-charged object was buried—in secret, probably at night—somewhere in the precinct would keep out trespassers more effectively than a barbed-wire fence. For a parallel, effective even to our own day, we may cite Shakespeare's epitaph, with its "Curst be he who moves my bones."

This hypothesis differs essentially from Professor Kent's only in inverting, instead of the vase, the eyes of the reader as he bends over it. The hypothesis accords with what is known of magical practices, in that it provides a mechanism for the attachment of the curse to a particular individual. It thus contributes an added support to Professor Kent's interpretation of this famous and difficult inscription.

CHARLOTTE ISABEL CLAFLIN

CAMBRIDGE, MASSACHUSETTS

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NOTE ON THE "EVOLUTION OF ARISTOTLE" AND  
CALOGERO'S *I FONDAMENTI DELLA LOGICA*  
*ARISTOTELICA*<sup>1</sup>

The philosophy of Aristotle has usually been studied as a closed system without regard to the conjectural dates of his writings or the development of his thought. Of late, however, it has become the fashion to say that the next task is to determine the chronology of his works and trace the evolution of his philosophy. I wish first to define with dates my own relation to this tendency.

In my doctor's dissertation *De Platonis Idearum Doctrina*<sup>2</sup> I pointed out that Aristotle was unable to maintain consistently in his *Metaphysics* and *De Anima* his rejection of the Platonic idea, but was perpetually relapsing into a kind of bastard Platonism.

I also distinctly implied the opinion which I now hold that it will never be possible to resolve into a clear, coherent system the ambiguities and inconsistencies of the *Metaphysics*, the *De Anima*, and the metaphysical parts of the *Organon*.<sup>3</sup>

There are two reasons for this: First, that there is no agreement even today about the problems with which they deal; and secondly, that Aristotle himself never reached a stable and satisfactory conclusion.<sup>4</sup>

<sup>1</sup> *I Fondamenti della Logica Aristotelica*. By Guido Caligero. Firenze: Felice le Monnier, 1927.

<sup>2</sup> Munich, 1884, p. 6, n. 2.

<sup>3</sup> *Ibid.*, n. 1.

<sup>4</sup> *Ibid.*, pp. 5, 23, 28, 35, 39.

In my lectures on the history of philosophy at Bryn Mawr I taught that there were two Aristotles, the "empiric" Aristotle, source of Hobbes and belauded by Grote, and the metaphysical Aristotle whose relapses into virtual Platonism Grote and after Grote Gomperz deplored:<sup>1</sup> "So clear and so sound are these thoughts of his on the origin and function of general concepts that it appears at first incomprehensible how, after once gaining the shore, he could have slipped back into the vortex of doubt, and allowed himself to be engulfed in the depths of Platonic mysticism." This with increasing emphasis on the *Categories* and the *Topics* was the starting-point and the substance of my teaching of Aristotle at Chicago. The "empiric" Aristotle tried to get along with the doctrine that the ultimate units of reality are concrete individual material objects. The metaphysical and Platonizing Aristotle found that doctrine insufficient when he faced the ultimate problems of epistemology.

In an article published in the *New York Nation*<sup>2</sup> I definitely proposed to substitute for the systematic exposition of Aristotle's philosophy in Zeller and Gomperz a developmental treatment beginning with the *Topics*. Such an exposition I said would show that in Aristotle's extant writings we find a logic constructed mainly in a nominalistic and positivistic revolt against the Platonic doctrine of ideas, and a metaphysic and psychology which was constantly making concessions to and falling back into what was virtually the rejected doctrine. I then thought and still think it probable that this also represents the chronological sequence, but I did not and do not stress that point. The essential is that wherever and whenever Aristotle tries to meet the ultimate problems of metaphysics and epistemology he finds that the identification of absolute reality with the individual material object will not work.

In 1923 was published Professor Jaeger's book on the evolution of Aristotle. Its main thesis is that Aristotle's development was a consistent and continuous growth away from Platonism and into a strictly positivist and rationalistic philosophy of science. Professor Jaeger believes that it is possible to follow this process in detail by reconstruction from a few fragments of the lost works of Aristotle's youthful Platonizing period and by an analysis that will discover the parts of these lost works imbedded in the various rewritings of the major works as we have them. He is confident that he can reconstruct in this way the *Ur-Metaphysik*, the *Ur-Politik*, and the *Ur-Ethik*. However stimulating and suggestive some readers may find this hypothesis, the book contains much demonstrably false reasoning and misinterpretation of the texts. It has never to my knowledge been critically reviewed. I doubt if anybody has read it critically with constant recurrence to the texts cited except myself and my pupil, Dr. Mary Needler, a résumé of whose unpublished dissertation on the *Eudemian Ethics* will be found in *Abstracts of Theses, The University of Chicago*, "Humanistic Series," Volume V. Such Aristotelian

<sup>1</sup> *Greek Thinkers*, IV, 81.

<sup>2</sup> XCVI (1913), 77-79.

scholars as Ross and Burnet and Taylor could hardly have expressed even the vague assent to which they commit themselves if they had really studied the book.

In the spring of 1924 I lectured at the four Belgian universities on the evolution of Aristotle, criticizing Jaeger's book and setting forth more fully the theory outlined in the *Nation* article referred to above. Until I find time to revise the substance of these lectures for publication in English I shall occasionally call attention in notes and reviews to the facts and dates here given.

I now turn to the recent book of Professor Calogero on the foundations of the Aristotelian logic. Professor Calogero disclaims the intention to determine the chronology of Aristotle's writings to support any special theory of development. He is interested in the opposition running through the Aristotelian logic of two points of view which, though differently named, are broadly analogous to my distinction between the "empiric" and the Platonizing metaphysical Aristotle. He distinguishes what he calls the "noetic" and the "dianoetic" approach to logic. The noetic as the name implies is based on the direct (Platonic) apprehension by the *nous* of the unity of the idea, the concept, the notion, the *Begriff*, call it what you will. This would naturally lead to a logic of inherence or as I call it in my paper on the "Origin of the Syllogism"<sup>1</sup> a logic of intension. The dianoetic logic is the logic of the discursive intellect or the *understanding*, the logic of predication, of subject and attribute and, as I call it, the logic of extension.

The general analogy of this doctrine to mine is obvious. To note the divergencies and differences in detail would require a minuter study of Professor Calogero's book than is possible here. He applies the opposition of *noeticità* and *dianoeticità* to the entire logic and metaphysics of Aristotle with great subtlety and, so far as I have tested his work, with considerable precision of thought. What I shall make most use of when I return to the closer study of his book is his many illustrations of what he calls the *oscillazioni* of Aristotle's thought between the two methods and points of view. These oscillations he discovers with detective ingenuity and analyzes with infinite patience. But I shall use his examples chiefly as confirmations of my original thesis that it will never be possible to clear up the confusion in Aristotle's *Metaphysics* because Aristotle himself never cleared up that which existed in his own mind.

Professor Calogero, like Professors Ross and Burnet and Taylor, expresses timidly and vaguely his general approval of Jaeger's book. This is the more strange because he is not really in agreement with it, and though he is not himself interested in any theory of development, his entire analysis would support rather my interpretation. He is acquainted with my "Origin of the Syllogism,"<sup>2</sup> but regards it as dealing only with the obvious and less fundamental question of the nature of the middle term in the syllogism. That is not quite

<sup>1</sup> *Classical Philology*, January, 1924.

<sup>2</sup> Pp. 187, 189, 309.

fair. In his discussion of the quantity of judgments he has a good deal to say of the logic of intension and extension and would appear to say much more throughout his book if his terminology did not so often substitute for these terms such phrases as *l'unità della determinazione noetica, aderente, il punto di vista quantitativo*, etc.

Now of course the terms "extension" and "intension" are familiar enough. But Professor Calogero would not find it easy to show where before my paper they are systematically applied to the interpretation of the Aristotelian logic. In his historical and bibliographical review he censures many earlier Aristotelians for overlooking the distinction again disguised by variations in terminology.

However that may be, Professor Calogero's book exhibits great subtlety and acumen of thought and a real acquaintance with the Aristotelian texts. It merits closer study than it will receive. It is extremely difficult reading. There are many pages from which an Italian scholar who was not at the same time an Aristotelian specialist would not get a glimmer of an idea. Do you know what the "unità dinamica del sinolo" is? And can you translate "l'appercezione noetica come necessario presupposto della sintesi-dieresi dianoetica" or "e infatti l'asserzione della realtà del pensato, consona al punto di vista del criterio assoluto della verità raggiungibile per adeguazione, non poteva d'altronde considerarsi intrinseca a quella pura determinatezza dell'appercezione noetica (pur riferentesi, nel suo carattere unitario, alla medesima posizione fondamentale) sulla quale si fondava, nel suo intrinseco formalismo e intellettualismo superiore a ogni distinzione materiale di realtà e d'irrealtà, tutto l'ulteriore sviluppo della logica aristotelica"?

PAUL SHOREY

## BOOK REVIEWS

"Sind die *Dialoge* Augustins historisch?" *Silvae Monacenses Festschrift zur 50 jährigen Gründungsfeier des philologisch-historischen Vereins an der Universität München*. By ALFRED GUDEMAN. München—Berlin, 1926. Pp. 16–27.

Interested as the author of this paper has been these many years past in the *Dialogue* in general, through his fruitful preoccupation with the famous book of that name among the works of Tacitus, he naturally feels surprised and almost ruffled when he sees claimed for these productions of the youthful pen of Augustine a historicity which is universally denied every other specimen of the species from modern no less than ancient times; the more so when the claim of historicity, though without any very careful testing of the evidence, not merely holds the field with recent students of Augustine against Hirzel's very reasonable doubt (*Der Dialog* ii. 377, n. 3), but is supported by the great reputations of Bardenhewer and Schanz-Krüger. Accordingly he sets to the task of demolishing that view with great thoroughness and a certain unmistakable zeal and relish. The present reviewer, who, to be sure, was quite ready to accept the author's thesis even before the arguments were heard, and whose judgment in the matter at issue, therefore, may be discounted to that extent, feels that Dr. Gudeman has completely proved his point, and hence it will no doubt be sufficient to set forth merely the nature of the arguments that are employed.

These are: (1) Many other dialogues profess or presume to be a true reproduction of extempore conversation, such as those of Plato, yet we have the express testimony of both Socrates and Gorgias that things were put in their mouths which they had never thought of saying, and this *mos dialogorum* is expressly referred to by Cicero as a recognized convention. (2) The particular device with which Augustine seeks to secure a measure of plausibility for his contention, the stenographic record (*adhibito igitur notario . . . nihil perire permisi* [*Contra Acad.* i. 1. 4]; similar statements occur at various other points), is not an innovation on his part, but was employed by Jerome in his *Dialogus contra Luciferianos* (1: *visum est utriusque sermonem a notariis excipi*), which was probably written between 382 and 385, and in Rome, at a time when Augustine himself was in the capital city, several years before the date of the latter's own dialogue (November, 386, rather than 387, as Gudeman prints it), and no one, it appears, has ever argued for the historicity of Jerome's work. Of course it will not do, with Ohlmann, against whom the present polemic is particularly directed, to say that Augustine must have "lied," if his



statements are not taken at its full value, for so must Jerome and practically all other writers of dialogues as well. One might add that the fables and parables also, as "The thistle said unto the Cedar of Lebanon," etc., or "A sower went forth to sow," etc., are expressed in quite the same way, with a certain conventional technical untruth in the external setting. (3) The statement in the *Retractationes*, forty years later, that the *De Beata Vita* and the *De Ordine* were written between the separate books of the *Contra Academicos*, which is, to be sure, a mistake, is still one of a kind that might easily be made if the latter work were a fictitious dialogue, but almost, if not quite, impossible to make, if the works in question were strictly historical. (4) The length of one of the exact quotations from Cicero's *Academica*, which amounts to considerably more than a page and a half in Knöll's edition (iii. 7. 15 f.), almost certainly precludes the possibility of a mere conversation. Furthermore, the *perpetua oratio* is unusually long in these dialogues, and the material far too carefully arranged to allow us to think of it in each instant as an extempore effort. (5) The philosophical knowledge of the two younger disputants is altogether too extensive to have been derived exclusively from Cicero's *Hortensius*, yet this is the only work of the sort which they are supposed to have read. Their dialectical skill also is most astonishing. And here again, one may add, it would be useless to refer to the dialectical skill which Socrates exhibited in his debates, because, in the first place, this skill was recognized to be something phenomenal, and, in the second place, Socrates himself carefully prepared beforehand in writing his courses of questions and answers, so that later when he repeated them in conversation he knew very well just what he was driving at. This is very clear from the unequivocal testimony of Epictetus (ii. 1. 32, and my note thereon), who represents him as having written more than any of the philosophers, that is, while elaborating in private his own argumentation. But the elaborate dialectical precision of these dialogues is far beyond any beginner's extempore effort. (6) Again, it seems quite incredible that in actual fact Augustine should have gravely explained to his mother (*De Ordine* i. 32) that *philosophia* meant in Latin *amor sapientiae* when she has just been presented as philosophizing with her son, familiar with his reading, and aware that women seldom if ever were present in philosophical discussions. The definition here, one might suggest, has clearly another purpose than the enlightenment of Monnica, that is, it serves as a transition to the quotations from the New Testament with which the first book of the *De Ordine* closes. (7) Many of the details are ordinary τόποι of dialogues in general. (8) There are occasional inconsequences like *supra* frequently in place of *ante*, and the references within the dialogues to one portion or another by *liber*. (9) Finally, the style of Alypius is quite identical with that of Augustine himself, a circumstance which throws the gravest doubts upon the actuality of any stenographic reproduction.

Some minor points raised by the author I have passed over, as being of less evidential force, but, as I have already indicated, the main arguments

which he has presented seem to me to establish his contention beyond any controversy.

In this connection, since Dr. Gudeman is an American scholar who, because he has been residing abroad for more than twenty years, may not be as well known to the younger generation of scholars as to the older ones among us, I might briefly call to mind his principal scholarly activities of recent years. From 1905 to 1919 he contributed many important articles to the *Thesaurus linguae Latinae*, with the editorship of which he was for some time associated. Since 1911 he has written extensively for the *Real-Encyclopädie* especially on grammar, rhetoric, literary history, scholia, and critical symbols. Closely related to this sphere of interest have been his lucid *Grundriss der Geschichte der klassischen Philologie* (2d ed., 1909), and his extremely interesting *Imagines Philologorum* (1910). A whole group of minor researches have been connected with the *Dialogus* of Tacitus, which came to a focus in his monumental *Tacitus' Dialogus de Oratoribus*, etc. (1914, 528 pp.), the first edition of which had been published by Ginn and Company in 1894. In 1916 his *Tacitus' Germania* appeared at Berlin, which likewise had been preceded by an English edition (Boston, 1899-1900) of the *Agricola* and the *Germania*. The *Poetics* of Aristotle have also profited from his researches, especially along the lines of the early Syriac and Arabic translations (1919 and 1924), and in 1920 he published an important new translation into German of this work. Two spirited and close-packed manuals, also, a *Geschichte der lateinischen Literatur* in three small volumes (1922-24) and a *Geschichte der altchristlichen lateinischen Literatur* (1924), have come more recently from his tireless pen. A new and elaborate critical exegetical edition of the *Poetics* is assured for publication next year, and a second edition of the Allyn and Bacon *Agricola* and *Germania* as well as a critical bibliography of the *Poetics*, this last in co-operation with Lane Cooper of Cornell, will appear within the next few months.

In all it is a remarkable record of energy in research and zest of discovery and presentation, which is an honor both to the scholarship of Dr. Gudeman's native land and no less to that of his present domicile.

W. A. OLDFATHER

UNIVERSITY OF ILLINOIS

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*Fouilles de Doura-Europos (1922-23)*. Par FRANZ CUMONT. [Haut-Commissariat de la République Française en Syrie et au Liban, Bibliothèque Archéologique et Historique, Tome IX]. Two parts. Texte, pp. lxxviii+533; Atlas (124 plates). Paris, 1926.

A journal of classical philology is not the place for an extended review of this great monument of archaeological exploration, nor would the present reviewer feel himself competent to handle its details of archaeological discovery and exploration. But the book, which came to his attention through the channel of personal friendship, has seemed too valuable and important

for the general study of antiquity to be allowed to pass without mention in these pages. For it is much more than the report of the excavations of an ancient site. It is the opening of a new chapter in the exploration of the Hellenistic-Roman world outside of the great centers, an example and a program of what may be done in the East analogous to what has been done and is still doing in Egypt and in Africa. No classical or historical student can read unmoved the fascinating Introduction, in which the historical conclusions yielded by the excavations and by the meager ancient records are pieced together and set forth, with scholarship profound and acute, in style concise and brilliant. It was a novel sensation to the reviewer to trace the history of the ancient world during a period of three or four centuries with light focused upon one small outlying city, and to observe the elements in its situation which gave significance to a frontier post apparently casual and remote. Under Cumont's touch one sees again as living streams the great arteries of oriental intercourse, the rival claims, the rise and decline of Roman routes, the explanation of the wealth and power of the desert metropolis, the shrewdly neutral, Palmyra. New light is shed upon the early eastern campaigns of the Romans, and where historians (ancient and modern) have talked of the ambition and vanity of a Pompey, a Lucullus, a Crassus, reasoned statesmanship enters in. One gains a heightened estimate of Roman policy and administration, of its wise conservation of Hellenic tradition in the Eastern world, of its alliance with Hellenism to form against the chaos of barbarism a barrier, within which law and order and economic stability might give opportunity for peaceful living and cultural development. One sees in vivid example the forces of that perpetual warfare and chaos which surged outside of these barriers, breaking against its walls—sometimes making breaches in them, sometimes repulsed—but eventually overwhelming Doura, as in the end they overwhelmed Rome itself. One gains a new conviction of the immense significance to the world of the Roman capacity for administration, for law and public order; a new conviction of the tragedy of their decadence and loss in the vain rivalries of the third century, when warfare within played into the hands of warfare without, and prepared the collapse.

No classical scholar need feel ashamed to confess ignorance of the location of Doura-Europos, for its exact position has been so little known to geographers that it has been placed too far to the north and on the east bank of the Euphrates. It lies at the edge of the desert on a rocky bluff on the west bank of the Euphrates in Lat.  $34^{\circ} 45'$ , nearly due east of Palmyra, with which it was connected by a great caravan route leading to the Orient, the protection of which (as well as of river commerce) gave reason and importance to the site. The name Doura (corresponding to Germanic *burg*), indicating the rocky elevation upon which the place was built, is of Assyrian-Semitic origin, and the site—as one of naturally commanding importance—may well have been occupied long before Seleucus Nicator (312–280) established here a colony and gave it the name of Europas, from the Macedonian village of his birth.

But the name Doura (Dura) outlived the Seleucid designation, and in imperial times seems to have been the usual name (Τύχη Δούρας on one of the wall frescoes).

The most striking and spectacular discoveries at Doura were not made by these excavations, but preceded them, and were the cause of undertaking systematic exploration. In 1921 an English officer, Captain Murphy, found the remarkable frescoes which are the chief artistic embellishment of the *Report*. This discovery, communicated to Professor Breasted who was in the region, caused him to visit the site, and in the very brief time available to make photographs and to report on the site to the French government. These frescoes, published first by Breasted (*Oriental Forerunners of Byzantine Painting*, Chicago, 1923) and here reproduced in color, are of most extraordinary interest for the history of art. The detailed study and analysis of them occupy only a little less than half of Cumont's volume. The student of the history of painting and of early Christian mosaic will find here a veritable mine of information for the origin of motives and treatment, for the conventions of technique and the persistence of tradition. The frescoes in themselves are not of the highest order of art. They are the products, not of Hellenistic artists, but of native craftsmen, who had learned a conventionalized technique, partly oriental partly Greek, the origins of which reach back to the founding of the place in the Seleucid period. The summary of conclusions, extending from page 140 to page 164, is a fascinating essay upon Graeco-Syrian painting, reaching down to Byzantine times, which will appeal to many a reader of general interests, who would little suspect its presence in a detailed report of excavations. The finds of statuary and of other archaeological objects were slight, though Cumont's comments on a statuette of Venus, reproducing the pose and motif of the Venus of Capua, show strikingly how much light upon great problems of archaeology may come from finds of no great apparent significance.

To the general classical student nothing will seem more striking than the discovery of records (analogous to the business papyri of Egypt) written on parchment—mutilated but for the parts preserved perfectly legible. No earlier Greek parchments have been discovered than those found here, one reaching back as early as 195 B.C. It is probable that further excavation—for only two important points within the town were explored, the temple of the gods of Palmyra and the so-called temple of Artemis (Nanaia)—would reveal much more material of similar kind, or possibly even public records. What is true of Doura must also be true of many another site in the same territory, so that systematic exploration may look forward to the finding of documents which will be as fruitful for the economic and legal history of this vast region as the papyri have proved for Egypt.

The excavation of a city as considerable as Doura is not a work that can be accomplished in two or three short campaigns. Think, for example, of the hundred years and more that have been devoted to Pompei! Considering the

time and the resources devoted to it the results are striking—more striking certainly in the general historical picture of a remote city of the Roman frontier unfolded to our eyes than in any details. It gives a sense of reality to the significance of the *imperium Romanum* and the *pax Romana*, which a reading of Suetonius or even Tacitus sometimes tends to shatter.

G. L. HENDRICKSON

NEW HAVEN

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*The Minoan-Mycenaean Religion and Its Survival in Greek Religion.*

By MARTIN P. NILSSON. Acta Reg. Societatis Humaniorum Litterarum Lundensis. IX. Lund, 1927 (Oxford University Press). Pp. xxiii+582.

In the fifteen years that have passed since the appearance of Sir Arthur Evans' classical treatise on *The Mycenaean Tree and Pillar Cult* the archaeological material available for the reconstruction of the Minoan-Mycenaean religion has been prodigiously increased by finds both in the Aegean lands and in the more remote regions of the contemporary civilizations of Egypt and the Hittites. Minute study has filled the periodicals with hypothetical interpretations of this material and with hypothetical conclusions concerning the religion of the Minoan world and its relation to the religion of historical Greece. Professor Nilsson believes that the time has come for an orderly assemblage of the material and a critical review of the hypotheses, and he has successfully supplied these needs in the present volume.

As far as the satisfaction of the first need is concerned, it cannot be said that one can study the intricate subject with no help but Nilsson's book. It is illustrated with four plates and one hundred and thirteen figures in the text; but he has purposely refrained from reproducing all the monuments which can be studied in well-known and easily accessible publications. In use, therefore, the book must be supplemented with illustrations from other sources, to which, however, it provides a descriptive guide.

In his interpretation of the monuments he shows the admirable critical spirit already familiar from his other works, in which caution and open-mindedness are blended. He does not permit himself the luxury of positive assertion unless his evidence is beyond question; and he does not dismiss the wild ventures of others without sober refutation—though a suggestion of mockery is to be detected from time to time when the theory that he is dealing with is too extravagant.

An introduction of forty-five pages offers a brief summary of the history of the migrations, in which the most notable feature is the contention that the Mycenaeans were Greek invaders who had adopted the Minoan culture and religion. The body of the book, as its title indicates, is divided into two parts, the first, which is nearly twice as long as the second, being occupied with the

"Minoan-Mycenaean Religion according to the Monuments," and the second with the "Minoan-Mycenaean Religion in Its Relation to Greek Religion."

In his study of the monuments it is Nilsson's fixed principle to interpret them by themselves alone, from within. "It is our task," he says (p. 247), "to collect and sift this evidence without allowing ourselves to be influenced by analogies from Greek or any other religion or notices drawn from Greek authors, in order to lay a sound foundation from which we may proceed with some confidence in search of Minoan-Mycenaean traces in the historical religion of Greece." The only exception to this is found occasionally in the employment of analogies which the science of religion finds universally cogent. As might be expected, the ground for inference is more solid in matters concerned with the externals of the cult than it is in the exploration of the theological ideas which the monuments are supposed to portray. Indeed, in the chapter on the Minoan Pantheon one feels as if he were walking in a maze of conjecture, because here positive opinions of any kind are hazardous; but Nilsson never tries to foist his or other people's theories upon the reader by disguising the instability of their foundations.

Among the opinions advocated by the author, the following may be mentioned. After many theories of the double ax have been discredited, the conclusion is reached that this object was originally the sacrificial ax and hence, like the Christian cross, is partly a cult symbol and partly a cult object. The origin of the horns of consecration, it is believed, must remain uncertain, but their use and significance in the cult are well established. The pillars which have been regarded as aniconic representations of a deity or at any rate as peculiarly sacred in themselves are merely structural parts of a shrine; and as for columns, the fact that representations of them show a portion of the entablature indicates again that they have a constructive purpose and are probably representations of a shrine. The bell-shaped idols are representations of the goddess of the house-cult, who also appears in the form of a snake or a bird, the snake being not a representative of the dead but the protector and guardian of the house. This goddess and the Mistress of Animals seem to be the most clearly recognized Minoan deities. There is no reason to believe that the Minoans had but one great goddess, or that this goddess is identical with the Great Mother of Asia Minor, or that any Minoan goddess had a male consort as the Great Mother had. The sarcophagus of H. Triada represents neither the divine cult nor the cult of the dead, but the apotheosis of a dead king. The interpretation of the daemonic figures in the Minoan representations is especially open to doubt. "They are," says Nilsson, "not gods themselves but the stuff of which gods are made, daemons or beings of popular belief, roaming the land and haunting the sacred places or groves, superior to animals and to man and feared by him, but subject to the gods like the wild animals and like man the ministers of their cult" (p. 327). It might be possible to make out an equally good case for the theory that these figures represent some kind of ceremonial mummies.



These brief and bald statements certainly do some violence to Nilsson's carefully qualified opinions, but they at least present some of his more important conclusions.

In the second part of the book Nilsson is occupied with the question: "What is the Minoan and what is the Greek contribution to that product of fusion which is the historical Greek religion?" "This," he says, "is for the present the crucial problem of Greek religion." He begins by approaching the matter from the Minoan side. Certain well-attested features of the Minoan religion he finds still existing in later times. Some cult places continued in use and some cult objects; in three of the great cult places of Greece, Delphi, Delos, and Eleusis, a cult was already carried on in the Mycenaean age. He regards the evidence for the Minoan origin of Athena, Artemis, and Eileithyia as very strong, Athena being derived from the Minoan house-goddess mentioned above, and the other two goddesses being derived from the Mistress of Animals; for other goddesses the evidence is less satisfactory.

Next he searches for elements in the Greek religion which seem alien to the Greek spirit, and if he finds them attached to localities where there are traces of Minoan civilization, he regards the probability of Minoan origin as strong. The Cretan myth of the birth and death of Zeus he confidently claims as a Minoan heritage. Other instances of a Minoan divine child he finds in Ploutos at Eleusis; in Hyakinthos at Amyclae, a Mycenaean site (the name of the child here is Minoan); in Erichthonios at Athens; and lastly in Dionysus—undoubtedly a child in certain forms of the myth and cult, and ingeniously, but not quite convincingly, connected with Crete. He accepts the Euhemeristic explanation of the origin of the hero-cult advocated by Farnell and Foucart, and finds support for it in Mycenaean conditions; and he believes that the notion of Elysium, the Isles of the Blest, and the Garden of the Hesperides was inherited from the Minoans and survived by the side of the genuine Greek conception of Hades. He suggests that the deification of man, which is found in the H. Triada sarcophagus, is akin to the religious ideas of the mysteries, which were repressed in the ordinary Greek religion, and that the mystical desire to transgress the boundaries between man and god may ultimately derive from Minoan religion.

A brief notice can mention only a few of the more significant conclusions of a book of this kind and cannot give a just impression of the care with which the evidence is weighed. Those who find themselves in disagreement with any of the opinions expressed will yet find in the book great stimulation to further study and reflection, and all who occupy themselves at all with this difficult but fascinating subject will welcome it not only for the critical and comprehensive summary which it offers, but also for the imaginative insight which has discerned new interpretations and the wise caution with which they are presented.

IVAN MORTIMER LINFORTH

UNIVERSITY OF CALIFORNIA

*Aristophane*, Tome II, *Les Guêpes—La Paix*. Texte établi par VICTOR COULON et traduit par HILAIRE VAN DAELE. Paris: Société d'Édition "Les Belles Lettres" (Association Budé), 1924. Pp. 312.

Most of the serious errors and corruptions in the text of Aristophanes came into existence in the fourth and third centuries, between the time of the poet himself and that of Aristophanes of Byzantium who is thought to have made the first critical edition of the comedies. The mistakes that crept into the text in post-Alexandrian times have for the most part been removed by the aid of the scholia, Suidas, and the happy emendations of Bentley and other modern scholars. Little help, however, comes from the recently discovered literary papyri of the third, fourth, and fifth centuries A.D., for, although they confirm the conjectures of critics in some places, in general they offer the same text as that of our vellum manuscripts.

What then is the value of the Aristophanic tradition? Cobet and von Velsen maintained that little reliance could be placed upon the authority of the manuscripts of Aristophanes. Mazon, on the contrary, is convinced that the Ravennas and Venetus of Aristophanes are among the best extant manuscripts of all Greek literature, and that because of them the text of Aristophanes is one of the best preserved that have come down to us from antiquity. Coulon holds neither of these extreme views. On the one hand, he gives the fullest consideration to the readings of the manuscripts and refuses to set over against them preconceived rules of grammar and metrics; on the other, he does not follow the manuscripts blindly, particularly in the spelling of words, preferring the evidence of the grammarians and especially of the inscriptions to that of the manuscripts in this matter. Nor in the distribution of the verses among the interlocutors does he consider the manuscripts always a safe guide, because originally a change of speakers was marked in the manuscripts only by a dash or by double points, signs which were later replaced with the names of the characters by copyists and revisers, and consequently it is merely their personal opinion that gave us the traditional distribution of the verses, and no higher authority. The scholia preserve traces of discussions on this point among ancient commentators. A notable instance is the scholium on *Vesp.* 74-85: *τινὲς ἀμοιβαῖα· χαρίστερον δὲ λέγεσθαι αὐτὰ συνεχῶς πρὸς ἐνός*. This remark previous editors apparently overlooked. Coulon alone assigns the whole passage as a monologue to one slave, whereas all other editors divide it as a dialogue between the two slaves. Coulon's arrangement of the text is better, and has the scholiast's approval.

Again, it was the Alexandrian grammarians who put the names of Sosias and Xanthias in the list of *dramatis personae* of the *Wasps*, as Dindorf showed in the case of Demosthenes and Nicias in the *Knights*. For Aristophanes they are merely *οἰκέται δύο* (the reading of R and V), and they did not have distinctive personal names any more than the *θεράποντες* of tragedy had. Hence

Coulon properly designates them as οἰκέρης α' and οἰκέρης β' in the text; all other editors employ the names Sosias and Xanthias.

Coulon has made his own collation of the Venetus and Ravennas manuscripts through the use of the photographic reproductions of them, and has done the work so carefully and accurately that he is able in places to correct the previous collations of other scholars, and in some passages he even distinguishes between the readings of the first and the second hand. Thus his collation of R and V would seem to be the best there is.

In his choice of readings it is his independence of judgment that impresses one, his readiness to depart from the common view, as when, for example, he gives up some commonly accepted conjecture and returns to the reading of the better manuscripts (e.g., *Vesp.* 614), or adopts good emendations of others (*Vesp.* 48, 52, 218, 259), or contributes them himself (*Vesp.* 481), or derives a reading from a scholium (*Vesp.* 57) or from his own more careful collation (*Vesp.* Hypoth. 1 init.). He has given us a most excellent text, in fact, the best text of Aristophanes we have, if the impression made upon the present reviewer after dipping into it here and there be correct. Familiarity with the previous work on the subject and soundness of judgment mark his handling of his task. Yet there are of necessity some readings like the following where a difference of opinion will arise.

ἀναπελημένος derived from πεπληρωμένος of the scholiast is in itself a good suggestion of Coulon in *Vesp.* 108, but what does he understand the regimen of the accusative κηρόν to be?

Wilamowitz' conjecture ἦν κομίσαι which Coulon adopts in *Vesp.* 819 is altogether unnecessary and ill-suited to the context ἐν ἔτι ποθῶ besides. The manuscript-reading εἰ πως ἐκκομίσεις gives the sense required. Herodian too read the optative here. The use of the optative in primary sequence, i.e., after ποθῶ, finds support in *Av.* 121 and in multitudes of other examples furnished by the grammars. εἰ πως ἐκκομίσεις has almost the value of a wish inserted parenthetically between θήρῳ and τὸ τοῦ Λύκου, a wish that is the inner object of ποθῶ just as τὸ ἡρώων is its outer object. If the shorter form of the aorist optative active ending -αις, instead of the more common -ειας, should seem objectionable here, in spite of the occurrence of -αις in *Plut.* 1036, 1134, *Vesp.* 572, 726, *Lys.* 506, then some slight alteration of the manuscript-reading like that of Reiske and Brunck will suffice, but there is really no need to depart from the well-attested reading of the manuscripts.

Coulon adopts Blaydes' conjecture τοῦθ'· ὁ δ' in *Pac.* 25, thus breaking the verse exactly in the middle, whereas the manuscript-reading τοῦτο δ' both gives a satisfactory metrical pause and makes the slave refer to the beetle contemptuously as τοῦτο "this thing," a much stronger expression than ὁ δέ. The reading of the manuscripts is plainly superior.

Again, in *Pac.* 76 the reading of the manuscripts ὦ Πηγασίον μοι is undoubtedly right. μοι is found, to be sure, in late manuscripts only, but it occurs in the passage of Euripides that is here parodied, and besides it is used

frequently by Euripides in association with the vocative case, e.g., ὦ τέκνον μοι *Alc.* 313, *Orest.* 124, *Androm.* 747, *I.A.* 613, ὦ γύναι μοι *H.F.* 626, etc. Yet Coulon adopts Dindorf's conjecture ὦ Πηγάσειον, a decidedly inferior reading.

An estimate of the character and value of the French translation that accompanies the text will be left to those who are better qualified than the present writer to express an opinion on this point.

DUKE UNIVERSITY

CHARLES W. PEPPLER

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*Dithyramb, Tragedy and Comedy.* By A. W. PICKARD-CAMBRIDGE.  
New York: Oxford University Press, American Branch, 1927.  
Price: \$6.00.

Twenty years after his revision of Haigh's *Attic Theatre* for the third edition, Mr. Pickard-Cambridge, completing studies interrupted by the war, gives us this sane and useful book. It is the fullest, most convenient and orderly statement of the literary evidence for its three topics known to me. All current and most older speculative hypotheses are fairly discussed; those of Mr. Ridgeway on tragedy and Mr. Cornford on comedy at what seems to me unnecessary length. But the facts, the texts, are separately presented in their integrity and are never distorted by special pleading for or against a thesis. Lucid summaries from time to time restate the few positive conclusions which the author regards as established. A large part of the work is negative in the sense that it is critical and pronounces the evidence insufficient to sustain this or that of the many contradictory theories that have been proposed concerning the nature, origin, and history of the dithyramb; the origin of tragedy; and the evolution of comedy. Such phrases as the following are of frequent occurrence:

Page 5: "Its special connection with Dionysus . . . would not require discussion but for the attempt made by Sir William Ridgeway to disprove it." Page 10: "The attempt of Brinck . . . is very unconvincing." Page 12: "The balance of probability is against. . . ." Page 13: ". . . Is hardly tenable." Page 15: "Miss Harrison's interpretation has even less probability." Page 17: "But this seems to be very uncertain." Page 18: "At best it can be no more than a conjecture." Page 19: "There seems to be no justification for the statement of Wilamowitz." Page 22: "The proposal by Val. Rose . . . is sufficiently answered." Page 25: "It would be interesting to know how this is proved." Page 25: "Reisch . . . gives the date as 508-7, but I do not know why." Page 26: "I cannot agree with Wilamowitz." Page 27: "Wilamowitz seems to be interpreting Strabo in a scarcely justifiable manner." Page 27: "The conjecture . . . appears to have no foundation." Page 27: "He goes beyond the evidence." Page 27: "He denies the value

... but without giving reasons." Page 28: "But this is very far-fetched." Page 31: "Not as Smythe supposes." Page 37: "But in fact we cannot be certain." Page 38: "The suggestion . . . cannot really be sustained." Page 38: "Miss Harrison . . . with little justification. . . ." Page 40: "I can see no reason why Diehl should." Page 40: "We know too little . . . to have any right to generalize." Page 40: "This however is probably a mistake." Page 43: "His proposals are not very satisfactory." Page 44: "It is not easy to follow Comparetti." Page 48: "Our evidence . . . is not sufficient." Page 56: "There is no certain ground." Page 58: "It is an exaggeration to say."

Mr. Pickard-Cambridge apologizes for this negative tone. But his real opinion is to be found in a paragraph of the Preface which enunciates a doctrine that *Classical Philology* has preached for many years. "But I think it is one of the most important tasks of scholarship at the present moment—at least in regard to these subjects—to ascertain what can really be said to be proved or probable, and to draw the line sharply between history on the one hand, and attractive and interesting speculation, not founded upon evidence, on the other. It is with this end in view that these chapters have been written."

PAUL SHOREY

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*Senecas Apocolocyntosis. Die Satire auf Tod, Himmel- und Höllenfahrt des Kaisers Claudius. Einführung, Analyse und Untersuchungen. Übersetzung von OTTO WEINREICH. Berlin: Weidmannsche Buchhandlung, 1923. Pp. xi+149.*

Professor Weinreich's previous publications in the field of religion and mythology prepare us for his interest in Seneca's famous skit. One aim of this volume is to arouse a more intelligent general interest, without omitting the mooted points with which the scholar is familiar—or unfamiliar. Hence the character of the Introduction, the giving of a translation rather than the original, and the fulness of the exposition. At the same time Weinreich has given a thoroughly formal analysis, a very adequate discussion of the *genre* with relation to the Menippean background, and has advanced the case for Seneca's authorship, if that were any longer needed. It was an unfortunate decision not to give the few pages of the Latin as it appears in the Bücheler-Heraeus edition of Petronius (1922\*). The space problem could have been solved with double gain by pruning a style which is often very discursive and repetitious. Four times we are told in brief compass that the speech of Augustus is a climax; and repeatedly, that the occasion for the "release" was probably a *cena*. At any rate, overemphasis of the whimsicalities is not the best way to reveal Seneca's cleverness. Compare (p. 17): *primum si noluo*,

*non respondebo. Die Maske ist abgeworfen, der Buffone steht vor uns, der den Leser genasführt. Lächelnd, mit überlegener Geste, fährt er fort: quis coacturus est?*

But the author shows much acumen in interpretation and sanity of judgment. The Bücheler tradition is not always followed (e.g., his famous *Tiburi* vii. 4 is rejected). Most of the *cruces* are fairly approached, so that the layman knows of their existence, and the scholar knows what has led Weinreich to his position. The discussion of the literary *genre*, for which Helm's study<sup>1</sup> has furnished much material, is particularly detailed and valuable; but I cannot accept the reasonableness of so formal an analysis of the whole: A. *Dreiteilige Overtüre* (Proëmium, episch-parodische Exposition, Vorspiel a, b, c); B. *Erstes Hauptstück* 1, 2, 3a, b a, β, γ; C. *Intermezzo*; D. *Zweites Hauptstück* 1, 2; E. *Exordium*. This may prove that the traditional end of the manuscripts is the real end, but most favorable critics have rightly seen here an unstudied style, a "literature of escape" in more senses than one. Its wit flashes brilliantly in the title, which would be dulled by any further reference to it at the end, sparkles along irresponsibly, interspersed with the neat projection of Nero's praise, the seriousness of the indictment by Augustus, and the *tour de force* of the *nenia*, as the high spots, ending with comic application of Seneca's sentiment: *quod quisque fecit, patitur* (H.F. 735). Formal analysis may be applied fruitfully, for those who must have it, in the *nenia*, where the categories of Menander for the ἐγκώμιον βασιλέως are fairly well observed. Seneca had doubtless strictly conformed to similar requirements in the *laudatio* pronounced by Nero.

The translation, polished by many hands, is in some measure a composite. The rendering of *vos mera mapalia fecistis* (ix. 1), *aber ihr führt euch ja auf wie die reinsten Kaffern*, attributed to A. Marx, is at least justified by the provenience of *mapalia*, and is therefore better than the *wie im polnischen Reichstag* of Weinreich's comments (p. 87). *Bei Gott* for *mehercules* (viii. 2) loses the comic effect of a colorless oath in the presence of Hercules himself. *Illi collum praecidi* (vi. 2) is playfully overdone by *einen Kopf kürzer zu machen*. *Den Hals abzuschneiden* (Stahr) had grim accuracy. *Seht er ist da, hallelujah* (xiii. 4) and *diese Ehre dürfe man nicht Krethi und Plethi (vulgo) geben* (ix. 3), illustrate efforts at spirited effects. The translation most nearly resembles Stahr's, as far as my knowledge of German translations goes, but is a vast improvement. In general it lacks the delicacy of Ball's English translation. A double entente is apt to lose its better half.

The book represents a good tendency in Germany, a work of sound scholarship, not entirely for the technical scholar.

O. F. LONG

NORTHWESTERN UNIVERSITY

<sup>1</sup> *Lukian u. Menipp* (Leipzig, 1906).



*The Use of the Optative Mood in the Works of St. John Chrysostom.*

Catholic University of America dissertation. By REV. FREDERICK WALTER AUGUSTINE DICKINSON. Pp. xvi+179. Washington, 1926.

This is Volume XI of the "Patristic Studies" published by the Catholic University of America. These "Studies" deal chiefly with the revival of letters in the fourth century as revealed in the language and style of the Greek and Latin "Fathers." Certain aspects of the writings of SS. Basil, Augustine, Ambrose, and John Chrysostom have been treated in the earlier numbers of the series. In this one we have a statistical study of one of the cardinal changes in the Greek language of the early Christian centuries. A normal test for the literary quality of a late writer's style and of his divergence from Attic standards is his use, disuse, or misuse of the optative mood. Its forms are comparatively rare in inscriptions, in the papyri, and in the New Testament, while from modern Greek they have disappeared. By the fourth century a large and fairly correct use of the optative was evidence of literary and rhetorical training. St. John Chrysostom, the most prolific writer and greatest preacher among the Greek Fathers, was a favorite pupil at Antioch of Libanius, the leading rhetorician of the age. That he should employ the classical language freely and with fair correctness according to the accepted Attic standard was to be expected. Our author counts and lists from the complete works of St. Chrysostom, excluding *spuria* and *dubia*, 6,025 occurrences of the optative and shows that in the majority of cases the usage conforms to the rules of Attic grammar, although the irregularities are numerous. These departures from the norm, it is suggested, may be due to a persistent indistinctness through the early Greek, and even occasionally in Attic, of the line between subjunctive and optative, or to the greater flexibility of usage in the changing language of the period, or to the unrestrained vigor and exuberance of so ready a speaker as the golden-mouthed saint. It is certain that he could use the optative with Attic correctness, but did not always do so. The commonest deviations from classical usage are: the future optative in wishes and in the potential use, the potential without *άν*, protases with the optative introduced by *έάν*, and the sequence of the optative after a primary tense. The introductory chapter discusses the vexed questions of the origin and development of the optative and of the encroachment of the subjunctive on the optative in the vernacular. There is also an interesting "Summary and Conclusion," a brief Bibliography, and an Index. But the great majority of the pages are filled with the thousands of citations and the statistical summaries. There is evidence of a prodigious amount of painstaking labor, yet the noting by the reviewer of an occasional slip lessens somewhat the impression of thorough accuracy without, however, impairing the larger value of the general conclusions. A few examples may be cited of a number of wrong or unprecise expressions: "connotated" (p. 4); the optative with *μή* does not indicate a

wish that one "does not desire to be granted" (p. 18); *βουλοίμην ἄν* is a substitute but not a "form of the optative of wish" (p. 35). More serious errors are to be found on pages 112, 122, and 164. Certain obscurities and an excessively repetitious formality in statement mar a piece of work which is a worthy contribution to historical Greek grammar.

H. A. HAMILTON

ELMIRA COLLEGE

*Troy and Paeonia: with Glimpses of Ancient Balkan History and Religion.* By GRACE HARRIET MACURDY. New York: Columbia University Press. \$3.75.

In this book Professor Macurdy has applied the resources of a fine scholarship to a patient and minute investigation of a number of problems centering in Homeric Troy and its relations to neighboring and allied peoples. The chapter-titles give a clear-enough idea of the scope of the work—"The God-Built Walls and the Builder Gods"; "Lykians and Sarpedon"; "The Close-fighting Dardanians"; "The Horse-taming Trojans"; "Trojan Names in the *Iliad*"; "Paeonia"; "Paeonian Sun-worship and Medicine"; "Helios-Hades"; "Paeon-Apollo and Poseidon"; "Artemis the Queen"; "Leto"; "The Averting of Evil—the Cock, the Sun, and Amber"; "Royal Names"; "The Hyperboreans"; "The Northern Muses and Spirits of the Water."

Mythology, ethnology, religion, and folk-custom all present problems which Miss Macurdy valiantly attacks; they are for the most part too minute for review in detail. In general the reader will find more of profit in the chapters which deal with ethnological relations, folk-customs, and place-names than in the interpretations of myth and religion. In the former field the author has added to our knowledge; in the latter her methods are too much like those of her admired fellow-worker, Miss Harrison, to gain the assent of cautious readers. For I cannot think that Miss Macurdy distinguishes as she should between an ingenious hypothesis and a proved fact. Thus (p. 110) she calls Miss Harrison's view that Helios and Hades are identical a "brilliant demonstration"; and because Admetos appears as an epithet of Hades (pp. 79 and 195) she accepts the opinion of Malten and Thomson that the Thessalian hero is an aspect of the gloomy god. But is not "Indomitable" natural enough as a name for a hero who is purely human?

The style of the book leaves much to be desired. The second sentence that begins on page 10 could hardly be worse. "A treaty has been made with victims" (p. 17) is ambiguous; and the closing sentence of the first paragraph on page 72 is awkward and breathless.

There are some misprints. *πορνόπιος* (p. 30) is decidedly unfortunate. On page 52 read *λείπεται*; on page 112 for Amazarba read Anazarba (though the form Anazarbos is preferred by Christ-Schmid). The length of the quotation from Homer on page 40 does not excuse the choice of a Greek type

which is much smaller than that employed elsewhere. In other respects the book presents a beautiful appearance.

CAMPBELL BONNER

UNIVERSITY OF MICHIGAN

*Josephus with an English Translation.* By H. ST. J. THACKERAY. Loeb Classical Library. Volume I: *The Life. Against Apion.* London: William Heinemann; New York: G. P. Putnam's Sons, 1926.

This, the first of the promised eight volumes of the Loeb Josephus, contains the two minor and doubtless the latest works of the Jewish historian. There is a brief introduction on the life of Josephus, his works and the manuscripts, also a map of Galilee and the surrounding district and two indexes, one general and the other of biblical passages. For nearly two hundred years the standard English translation has been that of Whiston, 1737. This enduring monument of a professor of mathematics at Cambridge passed through many editions and it appears with such regularity in old American libraries that in houses of some pretensions to education it would seem to have been one of the books "that no family should be without." The revisions of this by Shilleto, 1890, and Margoliouth, 1906, have held the field in recent years. The version by Traill, London, 1862, includes only the *Life* and the *Jewish War*. In Mr. Thackeray's volume the interest naturally centers in the *Contra Apionem*, which is a rejoinder to first-century anti-Semitism and has been said by an American Hebrew scholar to have become "for both Jewish and Christian writers the model of a systematic defence of the faith." Very valuable for us is the elaborate antithesis of Greek and Jewish beliefs and institutions. The statement in the introduction that "Domitian was the enemy of literature" will arouse dissent in spite of that emperor's suspicious repression of historians. While basing his text on the work of Niese and the Teubner edition by Naber, Mr. Thackeray has made an independent examination of the MS evidence and offers a few convincing emendations of his own. The fluent English of his translation has a distinction that does not belong to the plainer Greek style of Josephus. On the subject of graven images we are told (Ap. ii.12) that Solomon *πάσης ἀπέχετο τοιαύτης περιεργίας*. "Refrained from any (such) curiosities of art" is Mr. Thackeray's rendering. It is sometimes hard to escape the impression of *περιεργία* in this generally readable and accurate translation. On the whole students will be grateful for this project of a new and complete Josephus in English from the hand of so deft and conscientious a translator. In the long lacuna of Ap. II the text is supplied from the old Latin version. The sentence (II, 85), *neque enim extrinsecus aliqua ratiocinatione mentitus est*, has baffled all the translators. Mr. Thackeray renders doubtfully, "An outsider can make no sense of his lies." In turning *sed haec relinquo* (II, 102) into "But I refrain to pursue these inquiries" the English is for us of questionable propriety.

H. A. HAMILTON

ELMIRA COLLEGE

## BREVIORA

[The managing editor establishes this subdepartment because of the difficulty of procuring substantial critical reviews of all books, and the impossibility if they were found of printing them in our limited space. It is believed that brief but fair *complexes rendus* will prove more useful than a mere bibliographical notice. Contributions to this department should never exceed a page, and a paragraph is preferable.]

*Cicero, "Philippics," with an English Translation.* By WALTER C. A. KER, M.A.

*Cicero, "pro Lege Manilia," "pro Caecina," "pro Cluentio," "pro Rabirio," with an English Translation.* By H. GROSE HODGE. Loeb Classical Library. London: Heinemann; New York, G. P. Putnam's Sons.

The first of these volumes is rather old-fashioned, inconsistent, inaccurate, and dull. Italics are employed freely for emphasis; *Caius* and *Cnaeus* are used for *Gaius* and *Gnaeus* throughout; quaint words and expressions abound, such as "ah," "pray," "nay," "aye" affirmative, "alas," "O wretched fellow," and "holy Jupiter"; *toga praetexta* is "boyish gown" (p. 109); *mima* is "female mime" (p. 123) though often "actress"; *elusus* is "fobbed off" (p. 143); *P. Clodius* and *Publius Servilius* occur within five lines of each other (p. 77); plurals like *Luculluses* and *Serviliuses* (pp. 77 and 91) contrast with *Numisii* and *Seii* (p. 521); *municipium*, usually "borough," is "township" (p. 105). Particles such as *atque*, *quidem*, *equidem*, *vero*, *autem* are often rendered "indeed" without discrimination. A striking finale on page 98, *Tu vero adscribe me talem in numerum*, becomes "Aye! enrol me in the number of such." Where minor errors so abound it is needless to mention more serious defects.

Mr. Hodge's version is spirited throughout and really oratorical. The meaning is usually grasped with true judgment and rendered with correct emphasis. The ubiquitous superlatives, in particular, are handled with commendable taste and ingenuity. It is regrettable that the editors do not require that these translations be thoroughly modern in style. The present translator would be quite capable of avoiding such archaisms as "aye," "pray," "nay," "anon," and "bethink you"; the tradition of translation is old and tempts one to shirk the mental exertion necessary to write as the contemporary orator speaks. Yet it is a duty owed to young students. The Latin editors of this series have missed a great opportunity.

N. W. DE WITT

UNIVERSITY OF TORONTO

*Municipal Administration in the Roman Empire.* By FRANK FROST ABBOTT and ALLAN CHESTER JOHNSON. Princeton: Princeton University Press; London: Oxford University Press, 1926.

More than one-half of the six-hundred pages of this volume is devoted to original documents in Latin and Greek, numbered, listed, and indexed. Of

fifteen chapters seven were written by Professor Abbott. Roughly speaking, the later period of the Empire and the eastern parts have fallen to the share of Professor Johnson, who has succeeded so well in identifying himself with the plans of his lamented chief that differences of style are hardly perceptible. The same modest certainty of statement prevails throughout; no provoking conjectures are ventured upon where evidence fails. It is a model of scholarly diligence, organization, and collaboration, which will introduce specialists to all the literature bearing upon the subject to date, and will also constitute a dependable source of information for workers in allied fields. The volume takes its place at once as an outstanding monument of American classical scholarship.

N. W. DE WITT

UNIVERSITY OF TORONTO

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*Latin Epigraphy, an Introduction to the Study of Latin Inscriptions.*

By SIR JOHN EDWIN SANDYS. 2d ed. Revised by S. G. CAMPBELL, M.A. Cambridge: University Press, 1927.

This scholarly and well-documented work has been altered only by the omission of references to Wordsworth's *Fragments* and by additions to the references to periodical literature; on the latter side it is still deficient; there are a half-hundred journals containing epigraphical material of which scarcely any citation has been noted. One may mention the *Notizie degli Scavi*, though oversight is possible in such a multitude of footnotes. On page 188 in the Pompeian couplet the word *paries* should be printed *pariens* with an apex over the *i*; readings in inscriptions ought not to be changed when certain. Under the *praenomen* (p. 208) particular attention ought to be drawn to the fact that the spellings *Caius* and *Cnaeus* are fictitious and should never appear in English or Latin. The six long inscriptions at the end, easily accessible elsewhere, might well be replaced by collections of short inscriptions suitable for classes. One needs no knowledge of epigraphy to read the *Res gestae*, for example, nor does he learn any epigraphy from it. As a history the volume is extremely valuable.

N. W. DE WITT

UNIVERSITY OF TORONTO

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*Platons Dialektik des Seins.* Von JOHANNES THEODORAKOPOULOS. Tübingen: J. C. B. Mohr, 1927.

There are two reasons why I can only call attention to this essay and cannot attempt to do it justice in an adequate review. It practices the method against which I protested in my first Platonic papers,<sup>1</sup> the method of combining for a metaphysical conclusion bits of Platonic phrasing from different

<sup>1</sup> *A.J.P.* Vols. IX and X.

dialogues without explanatory exposition of the context in which and the purpose for which Plato employs them. Secondly it is written in a style of such abstraction as to be in my opinion inevitably equivocal and uncertain of interpretation. Here and there I catch glimpses of very rational meanings, but I can never be sure whether they are those intended by the writer. I can read my own theory of the idea of good into the sentence *als axiologischer Ursprung der Kultur gilt ewig die Idee des Agathon in all ihrer Kraft und Würde*. But I have no confidence that it is there. I have been reading philosophical and philological German all my life, but I do not know what the writer intends by translating ἄγρων πάθος of Philebus 15 B *unsterbliche Pathos*. I have a notion of what he means by *die Physis des Urteils besteht in der functionalen Durchdringung von zwei logischen Verschiedenheiten, die sich im unmittelbaren ἐξαιφνης vollzieht*, and by *die Weltseele darf also nicht als ein Weltwesen sondern muss als die Möglichkeit der Ideendurchdringung und Ausbildung des Weltalls verstanden werden*. But I don't believe many, even of his German readers, could stand an examination on these sentences, and I don't think that it is necessary to write philosophy in this style as the only alternative to writing it in the style of Dr. Durant.

PAUL SHOREY

*Primum Graius Homo*. An Anthology of Latin Translations from the Greek. Edited by B. FARRINGTON. Cambridge: The University Press, 1927. Pp. 64.

This is a convenient and beautifully printed collection, not intended to be complete, of the chief passages in Ennius, Lucretius, Catullus, Cicero, Virgil, and Livy that can be used to illustrate the manner in which the Romans translated Greek. The writer does not seem to be acquainted with Tollkühn. His Introduction and comments are generally judicious and sometimes illuminating. I do not believe that Lucretius' *necque nix acri concreta pruina cana cadens violat*, etc. pads objectionably or spoils the magic original and I doubt if Professor Farrington very seriously does. What would he say to Tennyson's "Where never falls the least white star of snow"?

PAUL SHOREY



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